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ORIGINAL ADDRESS.

THE GREAT ADVANCES IN SURGERY, AND HOW THEY HAVE BEEN MADE.

Being the Annual Address before the Academy of Surgery of Philadelphia, delivered January 5, 1885,

BY JOHN H. PACKARD, M.D.

MR. PRESIDENT AND GENTLEMEN,—Among the paintings in the rotunda of the Capitol at Washington, there is one which purports to represent the discovery of the Mississippi River by De Soto. Mounted upon a splendid prancing steed, the commander is advancing into a cleared space upon the bank; around him, in glittering armor, are soldiers, some of whom are dragging forward a brightly-burnished brass field-piece; beautiful Indian girls are presenting offerings of flowers and fruit; while chiefs and braves, in all the glory of feathers and paint, are looking on in stolid wonder at the scene.

It seems to me, however, that the artist has totally failed to apprehend or to set forth the true character and grandeur of the event. De Soto and his men, according to the most authentic accounts, had heard rumors of the great river, and had been for weeks struggling through the swamps and forests on their toilsome way in search of it. Weary and worn out, almost ready to give up in despair, they at length found themselves upon its banks, the object of all their labors and hardships attained. We can imagine the forlorn little band of adventurers, hungry and exhausted, with ragged clothing and rusty arms, but every gaunt face lighted up with the joy and exultation of success, as they gazed on the mighty current rolling towards the sea. Mighty as that stream was, it was less sublime than the human energy which had overcome every obstacle to find it, and whose servant it was thenceforth to be.

Other examples of like misconception of historical events might easily be adduced, and it may be that there are very many of them. Actions, perhaps in themselves trivial, which lead to momentous consequences, are apt to be magnified by the medium through which they are after-

wards viewed. When the Yankee farmer, crouching behind a fence, fired his old fowling-piece at the officer commanding a squad of British soldiers, he little imagined that the echoes of his shot would be heard all over the world. We see now that he began a contest which was to give freedom to unborn millions and to establish a vast nation, and we are inclined to rank him as a hero. Of all this he knew nothing. It is very possible that he heartily regretted his rash action; he may have even denied it, at times when the Continental cause seemed threatened with ignominious failure.

Much the same sort of illusion is apt, as it seems to me, to invest the prominent events in the onward progress of the science and art of surgery; and my purpose this evening is to consider these great advances, with the actual circumstances attending them. I have found it somewhat difficult to select a title which should define my subject without seeming to promise more than I could hope to perform. But indeed the actual number of such marked strides is curiously small,—smaller, I think, than is generally realized.

Another striking instance of the glamour with which past events are wont to be surrounded is to be found in the well-known picture in the École de Médecine in Paris, representing Ambrose Paré applying the ligature for the first time to check bleeding after amputation. The great surgeon stands in front of a patient whose leg has just been removed. Among the by-standers are several high dignitaries, one of whom offers Paré a heated iron, which he declines, holding aloft in his right hand a bundle of ligatures, which he is about to use.

No such scene, however, can be found described by the chief actor, who, we may be sure, would have put it upon record had it taken place. The honest old man was not averse to chronicling his triumphs, as well as the impression they made upon the spectators. As to his introduction and advocacy of the ligature he speaks at much length, but with as much modesty as conviction. The twenty-fourth chapter of his twelfth book is devoted to this subject. "Verily, I confess," says he, "I formerly have used to stanch the bleeding of members, after amputation, after another manner than that I have a little be-

fore mentioned: Whereof I am ashamed and aggrieved. But what should I do? I had observed my Masters, whose method I intended to follow, always to do the like." He then describes the suffering and the bad results which attended the use of the actual cautery, and concludes: "Wherefore I must earnestly entreat all Chirurgeons, that leaving this old and too too cruel way of healing, they would embrace this new, which I think was taught me by the special favour of the Sacred Deity; for I learnt it not of my Masters, nor of any other; neither have I at any time found it used by any: Only I have read in *Galen* that there was no speedier remedy for stanching of blood than to bind the Vessels (through which it flowed) toward their roots, to wit, the Liver and Heart. This precept of *Galen*, of binding and sewing the Veins and Arteries in the new wounds, when as I thought it might be drawn to these which are made by the amputation of members, I attempted it in many; yet so, that at first in my budding practice thereof, I always had my cauteries and hot Irons in a readiness, that if anything happened otherwise than I expected in this my new work, I might fetch succour from the ancient practice; until at length confirmed by the happy experience of an almost infinite number of particulars, I bid eternally adieu to all hot Irons and Cauteries which were commonly used in this work."

Against the new method of hæmostasis there was a particularly virulent objector, one Gourmelen, whose name is otherwise unknown. This man's words Paré quotes in his twenty-ninth book, and then, after some argument, relates several instances in which the ligature was used by his direction, in each case mentioning the operator by name. Unquestionably, had there been any such brilliant exhibition of the novel practice as the painter has undertaken to represent, it would have found a place among these; and the conclusion is unavoidable that it was imaginary.

Paré's official standing was so high, and his influence so great, that one might suppose a change of practice which had his sanction would have been at once effected wherever it became known. Moreover, the method was not really a new one. There are not wanting those who believe that Celsus, who mentions the ligature as a means of checking hemorrhage in acci-

dental wounds, had employed it also after amputation; and that he omitted distinct mention of it in the thirty-third chapter of his seventh book, in describing the removal of gangrenous parts, simply for the sake of conciseness. But Celsus was not only concise; he was also thorough, and would scarcely have left so important a matter to be inferred from other passages in his writings. And the plain meaning of the concluding sentence in the place above referred to is, that after the first dressing, amputation-wounds should be treated like others in which suppuration is to be expected.

So far was the great and practical step advocated by Paré from being generally or immediately appreciated, that nearly two hundred years after the operations recorded by him, we find Samuel Sharp, Assistant-Surgeon to Guy's Hospital, speaking of the actual cautery as follows: "Within these last fifty years this barbarous Practice has by degrees fallen into Disuse, both in France and England; but it is not absolutely discarded in every part of Europe." Hence it was at least one hundred years after a man, so eminent as Paré was recognized to be even in his own day, had made an urgent and really eloquent appeal on behalf of the ligature, before any general abolition of the cautery could be effected. Mr. Sharp, after presenting various arguments and combating various objections to the tying of arteries, thus concludes: "I hope I shall not be censured for labouring to establish a Point which no Man of Eminence in London contradicts. It is a sufficient Apology for me, that the Writings of the ingenious Surgeons I allude to, being in the Hands of our English Students, may possibly mislead them, if not warned against the Danger. Besides, Improvements of all kinds are so slowly propagated, that this, amongst others, is not Universally practised in the distant Counties of our own Kingdom; and therefore a further Enforcement of its Advantages will not, I believe, appear to be a useless Undertaking."

One would suppose from the words just quoted—the concluding ones of Mr. Sharp's original work—that he was a man thoroughly persuaded in his own mind as to the value and reliability of the ligature. Yet in his fourth edition there immediately follows a postscript, added for the purpose of noticing a new remedy,

—agaric,—with the declaration of opinion upon it by the French Royal Academy of Surgery, on the strength of which the inventor, M. Brossard, received from the king a gratuity and a pension. Mr. Sharp says, "As it hath not yet been sufficiently practised on the Femoral Artery, to warrant the Recommendation of it on large Vessels, in preference to the Ligature, I have forborne to speak of it in the Body of the Book. Should it hereafter be found equally secure with the Ligature, it would certainly be a most useful Discovery," etc.

This last passage seems to me to be singularly illustrative of a sort of almost superstitious credulity as to the special powers of medicinal agents, which stamped strongly the views of the older writers, growing, however, gradually less and less marked, until in our day it has given place to a scepticism which may indeed be far more philosophical, but which is certainly far less comfortable as a basis for therapeutics.

But I will ask you to let me recur again to the strange history of the matter we have had under discussion, which it is difficult for the modern mind—might I say for the American mind?—to comprehend.

Celsus, in the first century of our era, spoke of the ligation of arteries, in accidental wounds at least, not as being a new thing, but as a recognized procedure. How long it had been known before his time does not appear. It is mentioned again by Galen, and in the seventh century by Paul of Ægina. Yet not only these great men, but all their contemporaries, and those who followed them, continued to use vitriol buttons, cauteries, and like means for checking the flow of blood after amputations; it was reserved for Paré, in the sixteenth century, to extend the simple and familiar idea of the ligature to cases of the latter class. It would seem, indeed, as if this were but a small step to take, and one easy to follow. That it was not lost sight of, we learn from the writings of Le Clerc, Daniel Turner, and Gooch, in the eighteenth century. It must have been taught in the London schools; and yet scarcely a hundred years ago one of the surgeons of Guy's Hospital thought himself called upon to argue in its behalf, saying that there were in England many who had not yet abandoned the old and cruel practice.

It may be even more impressively stated

in another way. The ligation of arteries in accidental wounds was known in the time of Christ, but the idea was not applied to amputations until after the founding of the Royal College of Physicians in London, nor did this use of the ligature become universal until almost within the time of persons now living.

We are fain to think that such blindness at this day would have been out of the question; and, indeed, it seems inconceivable to the inquisitive, progressive, and practical spirit of our age,—impossible with the glare of light which modern customs and appliances have brought to bear upon all subjects of study, and with the existing facility of interchange of ideas. We flatter ourselves that had Paré lived in our time, and introduced such an improvement in surgical practice, the idea of the painter would not have been so very far out of the way, and the great step would at once have been taken with all the *éclat* with which he imagined it to have been surrounded. But we must not forget that our standpoint is very different from that of his contemporaries, and should have charity for their conservatism.

The science and art of surgery have been built up, as we now have them, by observation and invention. Yet this expression is hardly correct. In the construction of a building the workmen are guided by a plan. The architect has a definite idea in his mind of what the result is to be, and to the carrying out of this every man's task is subordinated. No such plan can exist in a science which has to deal with constantly varying conditions, with an incalculable number of combinations; or in the art associated with that science.

We have, however, a vast mass of experience, accumulated and recorded by hosts of workers, past and present, and marshalled into a sort of order by those whose abilities and position have qualified them for the task. Certain general principles have thus been evolved, which constitute the body of surgical science. Yet if one were to undertake to set down absolutely unquestionable doctrines or principles only,—fixed facts, which it is certain cannot by any further degree of research be changed or set aside,—he would find that the resulting material would be of very small bulk indeed. What had seemed to be a stately building would turn out to consist of a very few stones, with a shapeless and

irregular, because temporary and mutable, scaffolding over them.

This statement may appear to be very disparaging to the immense amount of surgical literature; but in fact this is made up largely of history, and of quotation of the recorded experiences and opinions of others. Especially is such the case in this age of bookmaking; and the difficulty is not that there is so much that is new to be read, as that it can scarcely be separated from the old with which it is mixed. The books which have really been great additions to the world's stock of knowledge have been but few, almost as few as the great advances in surgery, which we are now engaged in considering.

A great step in surgery is taken when a new procedure, adapted to various morbid conditions, is devised, or when a procedure already known is extended in its application to one or more classes of cases which are of frequent occurrence, and which have previously been looked upon as intractable.

Such a step was taken by the surgeon, whoever he may have been, who first amputated a limb for other cause than gangrene. Among the ancient surgical writers we find no mention of any such operation. Their idea was simply to separate dead parts, incising through them only, and trusting to the cautery to destroy what was left up to the still living tissues. In the Book of Deuteronomy, however, it is commanded that as a punishment for a certain immodest act, the hand of a woman should be cut off; and in the Book of Judges the maiming of captives by cutting off their thumbs and great toes is spoken of as having been done in seventy-one cases. In neither instance is there any explanation given or comment made, and we are left to infer that operations of this kind were not unfamiliar to those who received and preserved these writings. One can scarcely help speculating whether the civilization of the ancient Egyptians, Persians, or Hindoos may not have included surgical skill in the treatment of the accidents met with by those engaged on their vast architectural works. That they had mechanical knowledge which perished with them cannot be doubted, yet this must have been familiar to great numbers of workmen, and

continually seen by the people, whereas medicine and surgery, being in the exclusive possession of the priestly or other privileged classes, and being of necessity private in their exercise, would have been far more likely to be lost with the decadence of the nations. Perhaps we are too ready to assume that a basis of anatomy and physiology must have been wanting to any such ancient system of surgery, although the tendency of the religious beliefs of that day, so far as we know them, was strongly opposed to the desecration of the human body involved in its dissection. Possibly there were burnt in the Alexandrian Library records which would show that our most recent improvements are really only revivals of things familiar to the physicians of the Pharaonic times. All this, however, is of course mere conjecture.

With the loss of simplicity in habits of life, with the introduction of fire-arms, and in the present century with the invention of steam machinery, the occasions of amputation have very largely increased, and improvements in its mode of performance were inevitable. But not one of these changes has equalled in magnitude the first step,—the demonstration that a diseased or injured limb might be removed without waiting for its death, and that by this removal the life of the whole organism as such might be preserved.

In connection with the subject of amputation, and recurring for a few moments to the checking of hemorrhage after it, let me remind you that both Galen and Paul of Aegina advise that if a wounded vessel be large it should be seized with a hook, stretched, and twisted moderately. It seems to me that this expression could hardly refer more distinctly to the tenaculum as we now have it; yet by some modern writers the invention of this instrument is credited to Bromfield, in the last century.

And let me digress for a few moments to refer to certain improvements which, while valuable, cannot be regarded as great steps in the true sense of the term. One instance only will suffice.

Paré figures what he calls "the crow's beak, fit to draw the vessels forth of the flesh, so that they may be tied." This instrument was merely a pair of nippers with flat blades, ridged or roughened at their inner sides near the ends, so as to give

them a secure hold. The opening and closing of the blades required a separate movement of the hand. Le Clerc speaks of forceps having a spring, or the *valet à patin*, this latter being a ring slipped down over the blades, so as to keep them closed. The spring was perhaps the invention of Assalini. Then it occurred to some one to make the two branches spring outward, so that the instrument was open unless pressed together. A further advantage was gained by adding a slide, so that the blades, brought together by pressure between the thumb and finger, could be fastened thus, the whole process being effected with one hand. And by making the ends bulbous, the risk of including the instrument in the ligature was done away with. For special purposes, the ends were very early made toothed.

Now, here was a succession of improvements, resulting in a very perfect instrument, having a very wide range of application; and yet neither the whole process nor any one of its successive steps could be at all regarded as a great advance in surgery. There was no great principle involved in either the construction or the use of the instrument; it was a mere device for subordinate purposes.

To return for a moment to the subject of amputation. When the idea was conceived that the soft parts might be so divided as to insure an ample covering for the bones, it was so much of an improvement as almost to come within the range of our great advances in surgery. But it was not new; it was only a suggestion for the carrying out of a principle which was abundantly recognized already. And the same may be said of the flap method, with all its subsequent modifications; the admirable stumps so often obtained by the circular incision, when properly carried out, have not been surpassed by the results of operations done with the flap, which in fact has only the advantages of greater convenience, of more rapid execution, and sometimes of adaptability.

The introduction of excision of bones and joints was properly a great step in surgery. Its history is curiously like that of some other operations of high value. Filkin, who in 1762 excised the knee-joint, was without followers for nearly twenty years, when his procedure was imitated by Park, with a success which made him an

enthusiastic advocate of the measure, and led him to extend its use to the other articulations. The Moreaus, father and son, in France, practised it largely, and, I believe, with gratifying results. Other French surgeons imitated them, and later the operation found promoters in Liston, Syme, and other British operators. In this country, although many joints have been excised to the saving of useful limbs, we may claim more in the way of successful excisions of bones, beginning with the brilliant achievement of Dr. John Rhea Barton, of this city, in 1826, on the ankylosed hip of a sailor. For many of the deformities due to ankylosis, as well as to distortions of the bones, the more modern operation of subcutaneous osteotomy answers better; but I shall have occasion to mention this again.

I am aware that there are not a few surgeons who are inclined to regard the excision of joints with disfavor, on account of the trouble and risk involved in them, and the uncertainty of the ultimate result in any given case. Yet the many instances of perfect recovery with useful limbs,—far more useful than any artificial substitutes could ever be,—and hence the inference that due care only is needed in the selection of the patients to be submitted to such operations, warrant us in assigning to the introduction of these procedures a place among the great advances in surgery.

I shall next refer very briefly to a great step in advance, made by one of the ablest men who has ever adorned the profession of medicine. When John Hunter, in 1785, tied the femoral artery for a popliteal aneurism, he initiated a practice destined, in various forms, to be of vast service in the treatment of a disease for which there had previously been no known remedy. He based the procedure upon a thorough knowledge of anatomy, and upon the most philosophical reasoning; and since his first attempt, which was crowned with brilliant success, the labors of subsequent surgeons in this field have been almost altogether in the direction which he indicated. Instrumental compression, digital compression, and other modifications of means for carrying out the great principle which he laid down, have as yet yielded the best fruits. It would be foreign to my present purpose to speak of the experiments which have been made in other lines, even

if they had been more extensive or more satisfactory than they have yet proved.

I will barely mention subcutaneous surgery, which in the period of a little over fifty years since its introduction by Stromeyer has been so greatly extended in its uses, especially within the last two decades. It was certainly an advance of great importance.

Plastic surgery has made such advances in special directions, within the last half-century, as can scarcely be overlooked. I refer particularly to the operations devised, and successfully carried out, for the relief of vesico-vaginal fistula and of ruptured perineum. The limits of this field have not yet been reached. It is claimed that much good may be effected by suture of the lacerated cervix of the womb, and the already large number of cases in which it has been done with apparent benefit seems to warrant the belief that the claim may be substantiated. I presume there is hardly a surgeon who hears me to-night but has repeatedly been called upon to perform this operation, one of the newest on the list.

No enumeration of the great steps in our art would be complete without reference to abdominal surgery, and especially to its most important feature,—ovariotomy. I know of no more instructive portion of the history of medicine than that relative to the rise and progress of this procedure. Spoken of for two hundred years as a thing that might be done—perhaps, it was first carried into effect by a man whose merit was far greater than his fame. His teacher, the great Hunter, had suggested it in his hearing; and after his return to Kentucky, McDowell had the courage to attempt what others had only thought of. Three out of four lives did he save before he wrote an account of what he had done; and his modest statement was received with the utmost scorn and contempt by the then leading journal of the world.

Without knowing of Dr. McDowell's case, Dr. Nathan Smith, in 1821, performed an ovariectomy, and thus, although anticipated, may claim an equal originality.

I shall not detain you over the history of this operation during the next thirty-five years, but will merely say that the admirable account of it given by the late Dr. Peaslee will well repay careful perusal.

At about the end of this period, in 1857, the subject was brought up for discussion in the French Academy, and the following eminent names may be found among those of the most determined opponents of the practice: Boyer, Velpeau, Huguier, Moreau, Jobert, Malgaigne, and Cruveilhier. One only, Cazeaux, spoke of it as perhaps sometimes justifiable. The statistics at this time stood: in America, 97 operations, 54 successes; England, 123 operations, 71 successes; Germany, 47 operations, 13 successes.

One more statement: When one of the leading British ovariectomists, Mr. Spencer Wells, was knighted, a year or two since, he was complimented upon having added an aggregate of twenty thousand years to the lives of British women. I need hardly comment upon the facts now mentioned; but it seems to me that Ephraim McDowell, in the little town of Danville, in Kentucky, took a step in 1809 which entitled him for all time to rank among leaders in surgery; and if "Peace hath her victories, no less renowned than War," I think he may be assigned a place above many of those who have gained their laurels on the battle-field.

Of some other matters in abdominal surgery,—gastrostomy, cholecystotomy, nephrectomy,—it is as yet too early to judge. They promise, but they have not yet performed.

One great step will probably be taken at some future day in this direction, in the surgical treatment of intestinal obstructions. According to the *British Medical Journal*, July 19, 1884, the first definite and deliberate proposal to open the abdomen in search of an obstruction in the intestine was made by Barbette, in 1672. The first operation of the kind is recorded by Bonetus, in his "*Sepulchretum*," published in 1677. The first operation performed in England was by Sir B. Brodie, the abdominal cavity being opened in the middle line, for the relief of a hernia of the bowel through a rent in the rectum: it was unsuccessful.

Perhaps the greatest advance ever made in surgery was in the introduction of anæsthesia. The history of this wonderful event has been so often written upon, and so largely discussed, in view of the conflicting claims to its inception, that the mere mention of it is sufficient. It has been said that printing is "the art preser-

vative of all arts," and anæsthesia seems to hold a somewhat analogous position towards the rest of surgery.

Let me mention, as a curious fact, that rectal anæsthesia, generally considered one of the most recent of surgical expedients, was really proposed about forty years ago, and by no less an authority than Pirogoff. Although known to Roux in Paris, and experimented upon by other surgeons, the method was not long employed even by its inventor, although he occasionally continued to use it in allaying nephritic colic.

I would refer to what seems to be a great advance now in progress in the surgery of the chest, in the treatment of various affections of the lungs and pleura by surgical operation. This has the usual history. Krönlein states that in 1812 Richter asked, "Why should not lung-abscesses be opened externally?" and said that it had been done successfully by Benjamin Bell. Such was indeed the fact, and again an American was well up towards the front, for in 1823 Milton Antony published, in the *Philadelphia Journal of the Medical and Physical Sciences*, a case in which he removed, along with a diseased rib, a large portion of lung-tissue which was involved. Krönlein's case was that of a girl with a recurrent sarcoma, in which a zone of healthy lung-tissue was removed along with the growth, by means of scissors. The wound in the lung was united with catgut sutures, and in one month the patient was dismissed cured.

I must not omit to mention the great advance in the surgery of the genito-urinary organs in the development of lithotripsy, the merit of which was practically due to Civiale, or the almost equally great step made by our countryman, Dr. Bigelow, in showing the tolerance of the bladder, and experimentally proving that a stone may be crushed and removed at one sitting.

In special departments of surgery there have been some great advances made to which I can but allude, such as iridectomy, the use of the ophthalmoscope, and the correction of disorders of accommodation in the eye. The laryngoscope, and the means of removal of tumors in the air-passages, are instances of such improvements. But, valuable and important as they are, they belong so entirely to special departments that I cannot dwell upon them.

Perhaps some of you, gentlemen, will wonder why I have not spoken of antiseptic surgery, so called, as a vast advance upon all that preceded it. Others possibly will think that it need not have been mentioned at all. And it is precisely for this reason that I have left it to the last. It is still *sub judice*. Its able author and advocate has certainly shown the value of cleanliness, absolute as well as apparent. He has won over a very large and enthusiastic following to his peculiar views. Yet it remains to be seen whether the objects to be sought for, and claimed to be thus secured, may not be more effectually attained in other ways. The doubt thus expressed involves not only the method, but also the principle upon which it is based; and the time has not yet arrived for deciding the question.

I had intended, but time fails me, to speak not only of the great advances actually made in surgery, but of those also which have seemed to be such, but which have failed to stand the test of experience. Among these the most notable instances are acupressure, torsion of arteries (really of ancient date), syphilization, and *écrasement linéaire*. We are prone to judge hastily, to think we have found gold-bearing ore when the supposed prize turns out to be but spurious and fit only to be thrown away. Only very rarely is the precious metal found in ingots on the surface; generally it must be acquired by hard labor, and sometimes it is passed over unrecognized even by earnest seekers.

To one entering upon the study of medicine, the vast field which opens before him is apt to be bewildering. Yet as he becomes more familiar with it he grows more confident, until it seems to him that he has only to march in and occupy the land. To his ardent gaze, the problems which have puzzled the wisest heads and baffled the deepest research seem but to await solution. The untried energy burns to scale the heights, to triumph over the obstacles, to make the crooked straight, and the rough places plain. How different the scene as it is closely approached! and when, as this evening, we look back and see how few have been those triumphant advances, how slowly and with what difficulty they were effected,—difficulty not always inherent in the task, but born of envy or ignorance,—we cannot but wonder that there still exists such enthusiasm.

Yet enthusiasm is not the best of the forces for maintaining our onward struggle; and there takes its place, as one moves on, the sober conviction of the genuineness of certain principles, the love of science which is engendered by faithful work for it, and verily we may say that the last state of that man is better than the first. We "learn to labor and to wait,"—to do our duty as best we may, trusting to the future for our reward.

Mr. President, I cannot conclude without some reference to the memory of one who has gone from among us, whose noble presence and genial influence have often graced our meetings. With untiring energy his comprehensive mind was always gathering up whatever of new truth might be added to the common stock. He preserved in a most unusual degree the receptive spirit which is in accord with all progressive movement, and closed his long and useful career almost in the very act of presenting to the American Surgical Association a communication on an important subject.

I am sure that the recollection of our founder's influence, of his venerable face and form, will be ever cherished by those of us who have had the privilege of intimate contact with him, as incentives to earnest labor in the cause of surgical science, which was so dear to his heart.

ORIGINAL COMMUNICATIONS.

INVERSE TYPE OF TEMPERATURE IN TYPHOID FEVER, WITH A REPORT OF TWO CASES; TEMPERATURE-PECULIARITIES IN EPIDEMICS, WITH A REPORT OF SEVEN CASES IN ONE FAMILY.

*Read before the Philadelphia County Medical Society,
November 26, 1884.*

BY W. C. HOLLOPETER, M.D.

THE classical investigations of Wunderlich, Thierfelder, and Traube have done much to simplify the study of fever, and by their deductions they have rendered it possible for us to differentiate typhoid from all the other continued fevers by its temperature alone. This universal tendency to refer a given symptom to an established, or partially-established, law has done much to lessen the interest of individual work; for those cases marking

the exception are, as a rule, looked upon as complications, and slight causes are frequently brought forward to explain the apparent departure from the established rule. Dr. J. C. Wilson, in his admirably-written work on "The Continued Fevers," tells us that "marked deviations from the typical course of the temperature are always due to special causes. These causes, in many cases, cannot be discovered by the most searching investigation. On the other hand, upon inquiry, clinical facts of importance are often discovered, and it is therefore the duty of the physician, in every case when marked deviations occur, to make diligent search for their cause."

I enunciate a well-recognized fact when I state that every physician in general practice has had cases of typhoid fever where the temperature-record has been irregular, when, at the morning or the evening visits, the thermometer has registered the same, or has shown some unusual features in the evening exacerbation or morning remission, departing in some unsatisfactory manner from the gracefully-arched curve of Wunderlich.

It has frequently been my lot to treat cases of fever having every symptom of typhoid, yet not responding to the well-marked type of temperature as described by Wunderlich. For a long time it was very difficult to assign any cause for the variation, although it is natural for complications to disturb the rule, especially when we anticipate a typical curve in the temperature-chart.

Dr. Wm. Pepper has stated, in a recent clinical lecture, that "it was the exception, and not the rule, to find a typical case of typhoid fever." Although every symptom may be uncertain, or even frequently wanting, in some cases of typhoid fever, yet I believe the temperature, the peculiar form especially, remains the most constant factor, and, as Griesinger states it, "generally controls the situation."

During the last seven months I have had under my care a case of typhoid fever in which the temperature-record corresponded to the "inverse type," as described by Traube.

Bäumler, in drawing attention to this unusual character of the temperature, states that, in the great majority of cases, the daily fluctuations follow the rule of health, the exacerbation taking place in

the evening. We sometimes meet with cases where this order is reversed, the rise taking place in the morning and the remission occurring in the evening.

This "inverse type"—so named by Traube—of the daily fluctuations of a febrile temperature has been observed in some rare instances in typhoid fever.

I have been unable to find any additional reference to this unusual type of temperature in the systematic treatises on fever. Liebermeister and Murchison do not mention it in their works, nor have I been able to find any clinical reports bearing on the subject in any of the medical journals.

The clinical notes of my case are as follows:

Frank M., aged 14, of thoroughly healthy parents, was taken ill May 15, 1884. My friend Dr. Schoales, being the family physician, was sent for. The doctor was indisposed at the time, and I was requested to take charge of the patient. I found that the boy had been sick for two days, a slight chill occurring three days previous, and ever since he had been feverish, restless, and stupid; he had refused food and complained of headache; he had also slight nose-bleed and loose bowels. The history of the case was not unusual, having every well-marked typhoid symptom, and was of interest in the character of the temperature alone, which was as follows:

	A.M.	P.M.	Pulse.
May 15 . . .	102 $\frac{1}{2}$ °	102°	100
" 16 . . .	104	103 $\frac{2}{3}$	108
" 17 . . .	103 $\frac{1}{2}$	103	108
" 18 . . .	103 $\frac{1}{2}$	102 $\frac{2}{3}$	112
" 19 . . .	105 $\frac{1}{2}$	104	116
" 20 . . .	104 $\frac{3}{4}$	101 $\frac{2}{3}$	96
" 21 . . .	103 $\frac{1}{2}$	102 $\frac{1}{2}$	108
" 22 . . .	100	102 $\frac{1}{2}$	96
" 23 . . .	99 $\frac{1}{2}$	99	96
" 24 . . .	100 $\frac{1}{2}$	100	96
" 25 . . .	101 $\frac{1}{2}$	101	96
" 26 . . .	101 $\frac{1}{2}$	101	100
" 27 . . .	98 $\frac{1}{2}$	99 $\frac{1}{2}$	100
" 28 . . .	100	99 $\frac{1}{2}$	100
" 29 . . .	100 $\frac{1}{2}$	99	100
" 30 . . .	101 $\frac{1}{2}$	99	98
" 31 . . .	101	97 $\frac{2}{3}$	98
June 1 . . .	100 $\frac{1}{2}$	99	100
" 2 . . .	98	98 $\frac{2}{3}$	98
" 3 . . .	98 $\frac{1}{2}$	97 $\frac{1}{2}$	98

In this case the boy had been complaining for a few days, according to the statement of the parents, before advice was called in. How much longer he had been ill we are unable to state, but, judging from the elevation of the first temperature (102 $\frac{1}{2}$ °), the case was evidently well on in the first week.

The exacerbation recurring so regularly

in the morning, and the remission taking place as faithfully in the evening, caused me to watch the case with unusual vigilance. We could hardly be mistaken in the character of the fever,—Dr. Schoales subsequently visiting the case with me frequently and corroborating the diagnosis. The stupid and listless countenance; the distended and tympanitic abdomen; the rose-colored spots, few, but constant throughout the illness; the gurgling in the lower portion of the abdomen; the loose bowels; the nose-bleed; the deafness,—all conjoined to render the diagnosis certain. Then the duration of the illness, while not very severe, extended over four weeks, which corresponds to the average duration of uncomplicated typhoid fever.

The treatment of the case was such as to have but little effect on the temperature, except to lower it: diet exclusively of milk and milk food; the recumbent position faithfully maintained; large and well-ventilated room, thoroughly disinfected. No drug was used likely to influence the fever, with the exception that, during the prevalence of the highest records, thirty grains of quiniæ sulphatis were exhibited in ten-grain doses, two hours apart, during the night. This was ordered on three successive evening visits. Cold-water sponging of the body was continued frequently as long as the temperature remained above 102°. We were unable to discover any complications whatever; in fact, there were none,—no bronchial irritation, no disturbance of the alimentary tract. The course of the fever was so uniformly regular, the various symptoms occurring in their usual order, and the whole phenomena of the fever convincing as to its character, except the very important factor noticed in the *inverse type* of temperature.

The want of uniformity in the temperature-record of the case just noticed recalls the facts in a patient I had under my care nearly three years ago. The clinical notes recorded on the back of my temperature-sheet at the time are briefly as follows:

Walter, æt. 7, was a well-developed boy, and the youngest in an exceedingly healthy family. The mother informed me that the boy had commenced to manifest a disinclination for food and play five days before any trouble was suspected. I was therefore called in on the evening of the fifth day. I found the little fellow quite delirious, with a temperature of 103 $\frac{1}{2}$ °, a full pulse of 130, respirations 26, face flushed and apparently swollen, eyes congested, skin dry and hot, bowels confined. Ordered a fever-mixture.

In the morning temperature had fallen to 100 $\frac{1}{2}$ °, pulse 100. The patient had relapsed into a quiet sleep during the early morning hours. The skin had become moist. On a more careful examination I found the abdo-

men tender, somewhat tympanitic, and distended. The mother stated that there had been during the past ten days occasionally slight nose-bleed. The bowels showed a disposition to be free, yet, while not constipated, they had not been open regularly every day: now one or more free movements occurred daily. The third day, end probably of first week, if not more, the temperature, with a single exception, did not go above 102° , but had a gradual decline. The interesting feature was the morning record, which was higher than the evening. This unusual feature continued for twelve days. The amount of variation was from one to one and a half degrees.

On the twelfth day, about the twentieth of the attack, the morning temperature reached 103° without any other unusual symptom, after which it changed and ran as a typical case of typhoid fever in convalescence: morning 100° , evening 102° ; morning $98\frac{1}{2}^{\circ}$, evening $101\frac{1}{2}^{\circ}$, etc.

The downward curve was thus continuing uninterruptedly until the seventeenth day, when the temperature ran up to 102° again; following this rise the morning record marked 98° , and an intestinal hemorrhage of over four ounces of dark blood following an unusually large fecal movement.

The thermometer never registered a temperature after this above 100° . The case made a good recovery, convalescence slow. In this case we had a temperature-curve closely following an ordinary typhoid-fever chart, with the unusual exception of the twelve days of *inverse type*, with no discoverable complications to explain the cause.

I have not deemed it expedient to enter into an accurate analysis of each and every symptom of the foregoing irregular cases, knowing it to be exceedingly burdensome, yet I hope that I have sufficiently outlined the general features of typhoid, the phenomena of which are so constant as to make a diagnosis practically certain.

I wish now to present the salient points in a group of seven cases occurring in the form of an epidemic in one family, in which the victims followed each other to bed in rapid succession, four of whom suffered very severe and prolonged relapses. All, however, eventually made a good recovery. It is not my intention to narrate these cases as especially unique or exceptional, but as taking place under one roof and springing from one well-recognized cause: the variation in the temperature-record alone became an interesting element of study.

The fever occurred in a very healthy

German family; the parents as well as the children were free from any predisposing weakness.

Case I.—Mary, æt. 13, was the first to take sick. I made my first visit to her December 6, when I obtained the following history. The patient had been complaining for nearly a week of weariness, chilliness, aching in limbs, back, and head, loss of appetite, and a general disinclination to any physical exertion. I found her at 12 M. with a temperature of $104\frac{1}{2}^{\circ}$, pulse 112, skin hot and dry, puffiness around the eyes, with an injected conjunctiva, face dark and flushed. During the day had diarrhoea with vomiting. At 8 P.M. of same day vomiting (continuing) of thick, tenacious mucus, tinged with blood and bile; bowels were opened seven times during the afternoon. Since my morning visit the girl has had low muttering delirium, but can be aroused by speaking in a loud voice. The backache was so very severe as to merit especial mention, it being more pronounced than is usually found in bad cases of typhoid. The girl constantly complained of it during the first ten days. On the same evening, at 9 P.M., the thermometer had registered $105\frac{1}{2}^{\circ}$. In the morning it was $104\frac{1}{2}^{\circ}$, in the evening it was 105° . During the day and night vomiting and purging continued nearly hourly,—fifteen times by the nurse's report.

At the end of the eighth day, the second day of my visiting, the backache continued violently, the hands being involuntarily placed in that region. Bowels continued loose, vomiting the same. Distention and tenderness of the abdomen decidedly marked, with gurgling in right iliac fossa, tongue heavily coated, dark, and foul, sordes on teeth and lips. Vomiting discontinued on third day after taking her bed. Temperature $104\frac{1}{2}^{\circ}$ in the morning, 105° in the evening; low muttering delirium, alternating with violent outbursts; three or four well-defined rose-colored spots at this date made their appearance on the abdomen. After the fourth day, probably the tenth or twelfth day of her sickness, the temperature did not run higher than $104\frac{1}{2}^{\circ}$, but did not fall below 102° until the eighteenth day. One very noticeable feature in this severe case was that, for two weeks, every other morning's fall was less or intermittent in character, while the evening exacerbation remained the same. During this period there existed the wildest delirium, it being difficult at times to retain her in bed; bowels frequently moved involuntarily. The girl gained her strength very slowly; it was five weeks before the temperature approached normal, and it was ten weeks before she was able to leave her bed. She, however, made a good recovery.

The point worthy of attention is that the temperature in this nearly fatal case

remained nearly the same for the morning and the evening record. Immermann tells us that only in cases in which the fever is very severe and the absolute temperature very high is the difference less, and does not exceed three-fourths of a degree.

Case II.—The second member of the family to fall ill was the mother, æt. 32, who had acted as constant nurse to the daughter. It was thirty-four days after the daughter was taken sick, and while the daughter was yet in bed, that the mother was compelled to relinquish her duties. I might state that the mother was on the eve of her confinement, that she watched and waited on her sick child up to the hour of her illness; she passed safely through her labor, remained in bed one week, was up and around the house for *five days* before she had any decided symptoms of fever. She had a slight chill, which was soon followed by a temperature of $104\frac{3}{4}^{\circ}$ in the evening. This was, however, the highest point reached. She had no delirium nor vomiting. Bowels were opened daily, but not unnatural. The temperature remained high for over one week, ranging between 103° and $104\frac{1}{2}^{\circ}$. It had a gradual decline for over thirty-six days, when it reached normal, without any unusual variations between the morning and evening record. Convalescence was very slow. The record was a typical temperature-record from the second week of typhoid, yet much milder than the child. The puerperal state did not seem to modify the course of the fever or endanger the life of the patient.

Case III.—The husband and father, æt. 36, a house-carpenter by trade, rugged and compactly-built German, was next on the list. He was a perfect type of physical perfection. He continued his work up to the hour of taking his bed, February 10. He had complained of a cold, headache, and sore throat for two days previously. I found him on the morning of the 10th with a temperature of $105\frac{3}{4}^{\circ}$, pulse 90, respirations 20; dry, hot skin, dark and injected countenance; bowels loose, abdomen flat; very stupid and sleepy. In this case the pulse was full and regular, never going above 90, yet for over two weeks his temperature lingered around 105° , with profound stupor alternating in the wildest delirium. He had during the second week constant *subsultus tendinum* and frequent involuntary movement of the bowels. On the twenty-second day of his illness he suffered a hemorrhage of fully eight ounces. He had several smaller hemorrhages previous to this date. On the sixteenth day his temperature fell to $101\frac{1}{2}^{\circ}$, remaining under 102° with but one exception, when it ran up to 103° . Temperature did not reach normal until the thirty-eighth day.

Case IV.—Caroline, æt. 5, was taken ill on January 13, three days after her mother. She started with a temperature of $104\frac{3}{4}^{\circ}$. She had been listless and stupid for a week preceding her complete prostration; during the prodromata I registered her temperature twice daily, but did not find the thermometer above 100° . Two days from the last date the fever had reached $105\frac{3}{4}^{\circ}$. From this point on it was a gradual decline for twelve days, when it as gradually climbed up to its old figure of $105\frac{3}{4}^{\circ}$. This intermitting type continued for forty-two days, when it fell below 100° and I ceased to make a record. The case was one of unusual severity, frequently losing large quantities of blood by the bowel, yet constipation was the rule throughout the illness. Epistaxis was also constant, the face being stained daily with blood; low delirium existed for over two weeks. The patient was in bed for over seven weeks.

Case V.—Katie, æt. 10, had a temperature-record of her own,—a marked difference existing in this case from the others: above $104\frac{3}{4}^{\circ}$ to start with, reaching 105° the same evening, remaining above 104° for *three days*, then gradually declining for three days, when it assumed again the upward tendency, holding it for three days, then came a rapid fall. This intermittent character in the temperature continued for thirty-two days. This case was obstinately constipated throughout her illness, and was but slightly delirious. Recovery slow. Six weeks in bed.

Case VI.—Pauline, æt. 14, an unusually well developed girl, and the last case which I deem of sufficient interest to record in the history of this family-epidemic, will also illustrate a different phase in temperature-irregularities. Three of the children before they became actually sick were noticed to have a weary, listless expression, did not manifest interest in their play, showed a decided preference for the house, which was contrary to their usual habits.

I registered the temperature of two of the younger children for a week or more before they were stricken down, and I did not find an elevation of temperature but slightly above normal unless the temperature was taken in the evening, when it was generally near 100° . Taking into consideration the daily fluctuation in health, which is always higher in the evening, I could scarcely draw the line between the normal evening elevation in health and the insidious approach of the fever. With Pauline I still endeavored to anticipate the onset of the disease. Acting on the suggestion of some of my professional friends, I commenced to register her temperature nearly two weeks before I perceived any indications of her being the next to fall sick. I might anticipate any theoretical conclusions likely to find lodgment in your minds as to the

causation by stating that the girl had been employed in a dry-goods store, and was brought home to take charge of the sick family. Upon her devolved most of the washing and cooking for the sick. While the younger children were out of the house nearly all day at play, she was busy with household duties, and was in this way more exposed to the infectious disease than her younger sisters, who seemed to contract it so readily. I found it most convenient to take her temperature at the time I visited the other members of the family,—*i.e.*, between nine and eleven in the morning and eight and nine in the evening. I never found her temperature above normal until within four days before she was compelled to take to her bed. Three days before her prostration the thermometer registered 102° , normal the following day; second day it was $101\frac{3}{4}^{\circ}$; the following day it ran up until it reached $104\frac{3}{4}^{\circ}$. Her temperature continued high for over ten days, ranging from 103° to 105° , then assumed the intermitting character so frequently noticed in the record of the other members of this family, after which the fever-record had a gradual decline. By anticipating her attack of the fever and registering her temperature for over a week, three days in which her fever was above normal, gave us part of the ascending scale of Wunderlich, yet does not supply the gradual ascent and the lengthened arc of the semicircle which should describe the model typhoid temperature-record.

I have hinted that the cause of this epidemic had a tangible existence. While it is not the object of this paper to touch upon the etiology of typhoid, yet it may not be without interest to mention the environments of this fated household. It is unusual to have seven cases of typhoid occur in one family and follow each other in such rapid succession: yet not until the third member of the family was prostrated could an adequate cause be found.

The head of the family was an industrious carpenter, who resided, with his wife and five children (two older girls were not living at home), in a two-storied, four-roomed house near Thirteenth and Columbia Avenue. The house was quite comfortable for a small family, but not so for this one: hence they were crowded. Yet I have seen families packed in and live free from disease when there have been actually more to a room than in this family. The cellar was dry, drainage in a fair condition. A vacant lot of enclosed ground intervened between our family and the nearest neighbor on the north. This neighbor on the north was in the

milk-business, and for his convenience he had excavated a pit in the vacant lot adjoining our family's cellar-wall, four feet deep, into which he had dumped the rubbish of the yard. The rain and snow falling upon the decomposing mass of organic material soon found its way through the intervening stone wall,—percolated its liquid poison into the cellar. This filth, while not at all times sufficient to be recognized by the sight, was more frequently perceptible to the sense of smell. The pit could in no way impregnate the drinking-water of the family, for as a precaution I had all the water that was consumed for that purpose brought to them from a distant neighbor.

While we have mentioned the exceptional in the temperature-record, we may also entertain a doubt of this rubbish-pit being the only factor in the family illness. The first case in the epidemic, Mary, *æt.* 13, was employed in a store; hence she was not in the atmosphere of the house as long as the mother, who was the only one who constantly lived in the poisoned house and yet second on the list, and, while she had a severe attack, her illness was not so prolonged as the first. Again, Pauline, the eldest girl, came from another family in perfect health, resided for three months in the infected house, exposed to every form of contagion, and was the last to succumb to the disease; and when finally she was prostrated, the fever ran a comparatively mild course.

In directing your attention especially to the peculiarities of temperature in the foregoing cases, that ended in recovery, I wish now to contrast them with one that, while the temperature was under control and lower than any of the foregoing, yet, without complications, ended fatally.

A finely-built young man of 27, regular, but rather full habit, first complained August 1 of intense headache, backache, indifferent appetite, and general disinclination to exertion. He left the city, against the wishes of friends and my advice, for a trip through the South. On the 3d he was taken violently sick; sent home, reaching the city August 4. At my first visit, same day at 12 M., temperature was 104° ; at 8 P.M., $104\frac{3}{4}^{\circ}$. On the sixth day he lost consciousness. His delirium was wild and pugnacious; constant mutterings. At the same time his temperature fell below 104° , and remained below for over a week. On the tenth day his temperature reached $104\frac{3}{4}^{\circ}$. From this point it fell, and remained

under 103° until the fifteenth day, when it ran up to $104\frac{1}{2}^{\circ}$ at death.

It is unusual to find patients unconscious when the temperature is so easily under control. In this patient the only antipyretic measure used was the cold bath. None of the symptoms of this case were as severe as the group occurring in the epidemic; yet this case terminated in death, while all of the others, indifferently nursed and badly surrounded, made good recoveries.

While it has been my principal object to record these cases of typhoid as departing in a measure from the temperature-law of Wunderlich, I wish to call your attention incidentally to the following facts:

1st. Six of the group of cases noticed in this paper were children, yet we had a severe course of the fever and the temperature-record commenced high, showing frequent irregularities. Wunderlich states that in children, particularly in the younger subjects, the course of typhoid temperature is somewhat irregular. The commonest of these irregularities is its extreme mildness; yet the temperature rises in the first days to a higher average than in adults; it passes more quickly into the remitting period, and defervescence is less protracted, but complications often occur, closely indicated by the temperature.

2d. In the nine cases of typhoid, including mild as well as severe examples, we had four cases of intestinal hemorrhage, an unusually large percentage. Systematic writers on fever regard intestinal hemorrhage as a rare and grave symptom. While Liebermeister states that there is not a single symptom belonging to typhoid which can be characterized as pathognomonic, yet a tendency to diarrhoea is quite frequent and intestinal hemorrhage quite rare. In our cases we found the bowels confined in over half of the cases.

Dr. Broadbent looks upon constipation in typhoid as of sufficient importance to entitle the fever a distinct variety.

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ABSENCE OF TENDON REFLEXES IN DIABETES.—Professor Bouchard considers this phenomenon of great importance in the prognosis of saccharine diabetes. In forty-seven cases of diabetes in which the tendon reflex persisted there were only two deaths, or one in twenty-three; in nineteen cases with absence of the reflex there were six deaths, or one in three.—*Canadian Practitioner*.

REMARKS UPON THE MANAGEMENT OF LABOR.

Read at the Philadelphia Clinical Society, Nov. 28, 1884,

BY EDWARD R. STONE, M.D.

THE remarks which I have to offer this evening are based upon one hundred cases of obstetrics attended in private practice. This number is quite too small for the deduction of any accurate statistics, and they will therefore be used simply to illustrate certain points in the management of labor. The practice of obstetrics forms a large and important part of the everyday duties of most of us, and it is well occasionally to leave the discussion of rare and difficult cases to review the common things of our art. For these reasons you will excuse the following desultory remarks, which, while they contain nothing new, may serve to introduce profitable discussion. In the beginning, let me emphasize the importance of intelligent supervision of our cases as early in pregnancy as possible, which is apt to be neglected in the exigencies of other and more pressing duties.

It is not my purpose to discuss the details of the hygiene of pregnancy, or its various complications, except to bear testimony that in primiparæ great benefit follows preparatory treatment of defective and retracted nipples. We are all painfully aware of the not uncommon sequence of imperfect and ulcerated nipples, enlarged breasts, and mammary abscess, and the books and journals direct many and various plans of treatment, the very number of which is sufficient to indicate their untrustworthiness. The primary cause of trouble is almost always that the nipples are not in condition to fulfil their function, and efforts to continue lactation make a cure very difficult. At first I was timid about disturbing the nipples during pregnancy, knowing the marked sympathy between the uterus and mammary glands, but after some experience I have always found that the pregnancy progressed without accident. In cases in which the nipples are small and retracted it is well, for several weeks before labor, to direct systematic efforts to develop them. This may be done by gentle manipulations with the hand and suction by means of a glass connected with the woman's mouth by a rubber tube (one of the common kinds of breast-pump, such as is found in every

drug-store). At the same time the cuticle should be hardened with a solution of tannin and glycerin. Other astringents are recommended, as tea, brandy, or cologne-water, but all act in a similar way. Dr. Samuel Sloan, of Glasgow, suggests that glycerin and lead should be used with the astringent, to toughen as well as harden the skin. In the interval, pressure of the clothing should be removed by wearing a nipple-shield. In my experience, depressed nipples have usually been observed in girls of stout physique whose corsets and other clothing are made to fit tightly over the breasts.* If the physician has directed that he must be called as soon as labor-pains set in, he will be able satisfactorily to practise external examination. Later on in labor, the frequently-recurring contractions will interfere with its proper fulfilment. Lusk says† "that it is a good plan to employ external palpation in every case where no opposition is made, as, even where the diagnosis by ordinary vaginal exploration is clear and indisputable, no opportunity should be lost of perfecting one's self in mapping out the foetus through the abdominal and uterine walls. The ability not only to recognize the presenting part, but the position of the entire foetus in the uterine cavity, is, in many cases of difficult labor, a possession of priceless value." By palpation and auscultation can be ascertained—1. The fact of pregnancy. 2. The vitality of the foetus. 3. The probable position of the same in utero. 4. The suspicion, at least, of multiple pregnancy. By patient, firm, but gentle pressure with the hands laid flat on the abdomen, the parts of the foetus can usually be made out with considerable accuracy. The head can be readily detected as a hard, round body, usually at the pelvic brim, while the firm resistance of the back on one side is quite different from the irregular prominences of the extremities on the other. The point of maximum intensity of the foetal heart-sounds, which is always nearest the thorax, serves to confirm the diagnosis of position made by

auscultation. In the fourth position of the vertex I have several times been able to note the unusual prominence of the extremities, and the heart-sounds far around to the right side of the mother. A case which occurred some years ago, and in which Dr. J. B. Walker was called to my assistance, well illustrates the disastrous result of not recognizing a twin-pregnancy, which might have been done if external diagnosis had been practised.

Mrs. M., a slender Irishwoman, of medium height, æt. 38, was taken in labor at the termination of the sixth pregnancy. Nothing unusual was noticed, except that the uterus was very prominent. No external examination was made. The presentation was the vertex in the first position, and the cervix was soft and dilatable, the os about two-thirds dilated. The pains, however, were weak and ineffective. After waiting about two hours without much progress, I desired to use the forceps, but was not permitted to do so. Thinking that it was a proper case for the use of ergot as a uterine stimulant, two small doses were given with an interval of half an hour. The pains then became efficient, and a male child of rather small size (six and a half pounds) was expelled. As the birth was taking place it was ascertained that the womb contained another foetus, whose head could be felt in the right iliac fossa, and under the ergotized contractions the shoulder was forced down in spite of efforts to prevent it. The uterus continued to act violently, and the foetus perished before an anæsthetic could be administered for the purpose of version. As we were preparing for that operation, the foetus was suddenly delivered, probably by a process of spontaneous version.

This case reflects no credit upon the practitioner, but is none the less instructive, not only as showing the importance of using every means of diagnosis at our command in every case, but also as pointing a moral in regard to ergot, which has since deterred me from its use in similar circumstances. It is admitted that the diagnosis of twins before the birth of the first is difficult, and in some cases impossible, but in this case a careful examination would, I am sure, have disclosed enough to save me from the fatal error of administering ergot and thereby destroying the life of the second foetus.

This was the only case of multiple pregnancy which I have met with, and I have not therefore had an opportunity of testing the value of external diagnosis. The points in diagnosis of twin-pregnancy, as

* One may well wonder, also, whether the pernicious habit of ignorant nurses of rudely squeezing the breasts of newborn female infants may not have a bad effect upon the future usefulness of the glands. In two cases I have seen abscess result from this barbarous practice, and it is reasonable to suppose that the results of such abuse may leave the gland in a permanently crippled condition. If the mammary glands of an infant become swollen, it is well to forestall malpractice by covering them with small belladonna plasters, which will also allow nature to remove the trifling difficulty.

† Op. cit., p. 203.

given by Cazeaux, Lusk, Playfair, and others, are unusual size of abdominal tumor, a longitudinal depression in the median line, the detection of duplicate heads and other parts, and heart-sounds at two remote points of intensity. The difficulties in the way of external diagnosis are obesity of the mother, excess of the amniotic fluid, and contraction of the abdominal muscles due to the nervousness or timidity of the patient. The last, however, may be overcome with patience and tact on the part of the physician.

In the first stage of labor, the ordinary difficulty, in primiparæ at least, is from a lack of the proper dilatability of the cervix, the degree of rigidity varying from a mere temporary retardation of labor to those difficult cases which are only overcome by mechanical dilatation. In my cases, which show a rather large percentage of primiparæ (thirty per cent.), there were several requiring assistance, but none of a serious character. At first careful and systematic trial was made of the warm-water douche, but the results were disappointing, as it only washed away the secretions which nature throws out to lubricate the parts, and had no marked effect in relaxing the os. Chloral given in fifteen-grain doses until three or four doses were taken had, however, a most happy effect, especially where the rigidity was associated with marked suffering in an oversensitive cervix. In two cases, after the ordinary means had failed and the patient's nervous system was suffering from protracted anguish, a full dose of morphia hypodermically caused a temporary cessation of labor, which, after a refreshing sleep, was resumed and successfully terminated. In four cases, however, laceration of the cervix occurred sufficient to cause the usual symptoms subsequently. There may have been others, but these were all which were brought to my notice. Examination of some of these cases of imperfect dilatation has led me to believe that the maximum limit of normal expansion of the os may be insufficient to allow the head to pass, when a laceration is unavoidable, just as the vulvar orifice may be too small to give exit to the head without injury.

The proper management of the perineum occupies the anxious care of all obstetricians, and it seems that, even with the best-known methods, we have yet

much to learn. It is well that the old plan of pressure with a folded napkin is about obsolete, as it was erroneous in theory, and useless, if not harmful, in practice. The method of Professor Goodell accomplishes perhaps better than any other the three indications,—viz., to retard delivery, to assist extension, and, by drawing forward the perineum, prevent it from suddenly slipping back over the brow. It has seemed to me, however, open to the objection that in some cases the fingers in the rectum increase the tenesmic efforts and compel the patient to bear down more than is proper and safe. After having had a patient draw suddenly away from my hand at a critical period, I have, in some cases, adopted Dr. Jas. D. McGaughey's* plan of assisting extension and retarding delivery with the left hand passed between the woman's thighs from above. This can be done by having the patient on the usual left side near the edge of the bed, with the thighs separated, while the physician sits behind the patient, facing the foot of the bed. It has seemed to me that in this way the advancing head can be easier controlled, and the arm across the woman prevents any sudden and hazardous movements. The right hand is free to draw forward the perineum or to reinforce it in any other way. It has been my misfortune to meet with seven cases of rupture, all primiparæ; six of these were of moderate extent, not involving the sphincter ani. In one case the rent involved the rectum. It was, briefly, as follows:

Mrs. L., æt. 22, primipara, taken in labor about midnight, but was not visited until about 10 A.M. next day. The fœtal head was found to be well down in the cavity of the pelvis, the vertex presenting towards the sacrum. The pains were severe, but nature was evidently unequal to the task. Ether was administered and forceps applied. After considerable effort, delivery was effected by bringing the head into forced flexion. The perineum was torn through the sphincter ani, but primary union followed the introduction of sutures.

There is still diversity of opinion as to what constitutes a laceration calling for the use of sutures, physicians of considerable experience stating that they never had a case requiring other treatment than

* Am. Jour. of Obstetrics, June, 1884.

bandaging the knees together, while others often find use for sutures. The only safe rule is to make a careful examination of the parts, and if the muscular tissues and central tendon of the perineum are divided it is useless, in my opinion, to look for primary union unless the wound is firmly united. The legs may be tied, but the retraction of the transverse perineal muscles will cause the wound to gape.

The proper treatment of the third stage is of the greatest importance, but the history of the subject, unfortunately, teaches that the most diverse plans, from rude and forcible delivery to an expectant plan carried to an almost ridiculous extent, have had their advocates in high places. Not many years ago the teaching was to wait a half-hour, and gently assist nature by drawing on the cord; then came Cr  d  , with his many disciples of expression, and for several years this method has been the most popular. Quite lately, however, the pendulum of medical opinion has shown signs of swinging back again to less active means. Several have expressed themselves as being dissatisfied with Cr  d  's plan, because it empties the womb too speedily and is apt to leave behind the membranes to become a source of danger from hemorrhage and septic  mia. These either place themselves in an attitude of "armed neutrality" and wait for nature to accomplish the object, or adopt an intermediate plan which has received the name and sanction of the Dublin school. The latter is described* as following down the uterus with the hand as a sentinel and guard, and, as soon as uterine contractions are pronounced, assisting by firmer pressure.

Professor Stadfelt, of Copenhagen, presented to the late International Congress the results of the respective methods in a large number of cases. His conclusions were that Cr  d  's method is preferable to the expectant. Detachment and retention of membranes occurred somewhat more frequently after expression, but he thought that the objectors to it attached too much importance to the objections, and overlooked the fact that it removed dangers attendant on the time of waiting in the expectant plan. The cases treated by Cr  d  's method also showed a less per cent. of mortality and puerperal disease than did those treated expectantly. All

will admit that the patient is not safe until the placenta is expelled, which is sufficient ground for its speedy removal, if that can be accomplished safely. In the majority of cases the placenta will be found to be wholly detached very soon after, if not simultaneously with, the expulsion of the foetus, and can no longer serve any useful purpose, except to provoke uterine contractions to expel it. If the uterus has been followed down with the hand, there does not seem to be any good reason for not removing it as soon as the contractions are resumed and the blood in the uterine sinuses has had time to coagulate.

Dr. J. C. Reeve, in an historical and critical article upon the management of the placenta, expresses the opinion that Cr  d   wrote his own condemnation with the statement that in his practice the average duration of the third stage was only ten minutes. But it should not be forgotten that Cr  d   advised that the placenta should not be expressed until the uterine contractions are re-established, and this appears to me to do away with the objection that the method empties the womb too speedily.

It has been my custom to keep the hand upon the uterus from the time of the delivery of the shoulders until the third stage is completed, and for several minutes after, and as soon as a firm contraction is felt to remove the placenta by pressing and compressing the uterus with the left hand, while two fingers of the right hand are hooked into the edge of the after-birth which is usually protruding from the os, making traction and twisting up the membranes as soon as the placenta is brought outside of the vulva. Thus the left hand is assisted in the extraction, and the membranes are not liable to become detached, as sometimes happens with unassisted expression. I have been in the habit of administering a full dose of ergot as the head is emerging, in all cases other than primipar  . This practice is condemned by Lusk, Playfair, and others, on the ground that the contractions produced by the drug may imprison the placenta and lead to difficulty in its removal. This objection no doubt applies to those cases in which ergot is administered and the expectant plan pursued, but not if the after-birth is removed as soon as the pains are re-established. We are told by Playfair, and Wood in his Therapeutics, that the drug requires fifteen to

* Med. News, Oct. 4, 1884.

twenty minutes in which to act, which gives ample time for its removal by Cr  de's method or some modification of it. It is this tardiness of action which has influenced me in administering it early to forestall a possible flooding, for the patient may die exsanguine before the eccholic action of the drug can be secured. In my cases there was neither a single case of hemorrhage from inertia nor imprisonment of the placenta. Those who object to giving ergot before the completion of the third stage should be prepared to administer it hypodermically if symptoms of inertia come on.

The administration of ergot also has a favorable effect on the after-pains, by keeping the womb contracted, and gives the latter a start towards healthy involution.

The use of an  sthetics in obstetric practice is, perhaps, not as common in this country as it should be. This may be partly due to the national antipathy to chloroform, and the fact that ether is bulky, disagreeable, and liable to cause intoxication, during which the patient is difficult to control. Last winter Dr. E. E. Montgomery suggested to me the use of bromide of ethyl, and I have since administered it in eight cases, always with the very best results. The remedy certainly deserves a more extended trial, and I would commend its use to those engaged in the practice of obstetrics. Its advantages are—the small amount required to relieve pain; it does not interfere with muscular contractions, either voluntary or of the uterus; it does not seem to have a dangerous effect upon the heart, or to lead afterwards to uterine inertia.

In these days of bacilli and other microscopic germs of disease, obstetrics naturally comes in for its share of antiseptic treatment. The heads of the various great lying-in establishments have given us the details of carbolyzed hip-baths, removal of hair from the genitals, rubber gloves, antiseptic vaginal and uterine injections, and other means of excluding possible germs of disease, and some have gone so far as to accuse those who do not as they direct of culpable negligence. But it is well to remember that those to whom we look for instruction usually gather their large experience in maternities whose proximity to general hospitals provides a constant and fruitful source of septic  mia. Blockley Hospital is one of these, and I have seen

the time when scarcely a single woman delivered in that institution presented a normal puerperal period. But these precautions surely do not apply to private practice, if the practitioner is careful not to be a carrier of the germs of scarlet fever, erysipelas, or other similar disease. In attending considerably more than one hundred cases, I have yet to see a single case of septic  mia, although no other precaution than ordinary cleanliness was observed. In one hundred cases the mortality has been nothing, and the percentage of disease nine per cent. Three of these were mild cellulitis apparently due to lacerated cervix; one was a severe case of the same disease, which progressed to the formation of abscess. Recovery took place slowly, leaving the uterus fixed in the pelvis, but the woman after several months was able to work, and has fair health. The other cases, five in number, were mammary inflammation and abscess. This list does not include, however, abortions, nor deliveries before the f  etus was viable.

1539 NORTH NINETEENTH STREET.

REPORT ON MEDICAL CHEMISTRY.

BY WILLIAM H. GREENE, M.D.

A NEW LEVOGYRATE ACID IN DIABETIC URINE, PSEUDO-OXYBUTYRIC ACID. —E. K  lz and Hass, some time since, have shown that normal human urine rotates the plane of polarized light to the left, and that the left-handed rotation is still more marked with the urine of cattle, horses, and pigs. The rotatory power is diminished after treatment with lead acetate, but increased by lead acetate and ammonia. K  lz now finds that in severe cases of diabetes the urine possesses levogyrate properties which remain after treatment with lead acetate and ammonia, and which cannot be attributed to any of the hitherto known constituents of the urine. This he ascribes to an acid having the composition of oxybutyric acid ($C^4H^5O^3$), and which he isolates by the following process. The glucose is first destroyed by fermentation, and the urine concentrated to a small bulk and precipitated with neutral lead acetate and basic lead acetate and ammonia; the filtrate is freed from lead and evaporated to a syrupy con-

sistence. It is then treated with a little ninety-five-per-cent. alcohol, afterwards with absolute alcohol, and allowed to stand twenty-four hours. The clear decanted solution is then mixed with about five times its volume of ether, which precipitates the greater part of the new acid. The barium salt is soluble in alcohol, and may serve for the preparation of other salts which separate from their alcoholic solutions on the addition of ether. Analysis of the salts and of the free acid, which may be obtained in syrupy form by desiccation over sulphuric acid, gave figures corresponding closely with the formula of an oxybutyric acid, but the properties of the new acid do not correspond with those of either of the four known acids having that composition. It produces no color with ferric chloride, and does not volatilize in a current of steam. Külz has named it pseudo-oxybutyric acid, and has demonstrated its existence in the urine of all severe cases of diabetes where the presence of acetylacetic acid occasions the production of a Burgundy-red color on the addition of ferric chloride. The so-called *acetonuria* is possibly accounted for by the presence of this acid, which, according to Külz, yields acetone when oxidized by potassium dichromate and sulphuric acid, and is itself probably a normal oxidation-product of glucose. Also, the presence of this acid would explain the fact that the polarimetric estimation of glucose in urine often gives results considerably lower than those found by the use of Fehling's solution. If, after the fermentation of the glucose, a sample of urine is levogyrate, the reading to the left should be added to that previously found to the right.

O. Minkowski has continued his researches on the presence of oxybutyric acid in the urine of diabetes,* and has succeeded in further purifying the acid and preparing certain of its salts. He has also investigated its optical properties, and finds for the free acid a rotation of $(\alpha)_D = -20.6^\circ$; for the silver salt, -10.1° . Külz found for the silver salt of his acid above described $(\alpha)_D = -8.637^\circ$. Throwing aside its optical properties, Minkowski's acid closely resembles β -oxybutyric acid, and this investigator proposes to call it *acetonie*

acid. In all respects it corresponds to the pseudo-oxybutyric acid of Külz, and the two are undoubtedly identical.

Minkowski also considers that the acetylacetic acid obtained from urine by Jaksch was probably an oxybutyric acid. However this may be, it appears certain that the substances obtained by Stadelmann, Külz, and Minkowski are identical with each other, and have the composition of an oxybutyric acid, and that this acid is very frequently if not constantly present in the urine in grave cases of diabetes.

Minkowski believes, with Stadelmann, that diabetic coma is due not to the action of any specific basic substance, but to the accumulation of acids in the organism; Walter and Von Frerichs have energetically combated this opinion.

SOME NEW APPLICATIONS OF WEYL'S CREATININE REACTION. ACETONURIA.—The evanescent red color produced by the addition of solution of sodium nitroferrocyanide and sodium hydrate to solutions containing creatinine is usually known as Weyl's reaction. E. Legal finds that acetone and acetylacetic acid respond to the reagents just as does creatinine, with the difference that the brown-red color fades much more slowly and becomes darker on the addition of acetic acid, while it appears carmine-red when diluted. Diabetic urine which produces a red color with ferric chloride gives the same reaction, but ethyldiacetic acid behaves differently: its solution becomes brown-yellow when treated with the above reagents and acidulated with acetic acid, and is then straw-yellow when diluted. Legal claims to have detected acetone in the distillate of normal urine treated in this manner, and it would follow that, in the application of the test for determining the presence of creatinine, the urine should first be boiled.

C. Le Nobel has independently made an observation similar to the above. He finds that the ruby-red color produced by sodium nitroferrocyanide and strong sodium hydrate in solutions containing acetone is changed to violet by the addition of glacial acetic acid, and becomes green-blue when the solution is boiled or after it has been allowed to stand. Glacial acetic acid changes the color of the creatinine reaction to green. With ammonia and sodium nitroferrocyanide, acetone gives first a rose-red, then a violet or wine color;

* See Medical Times, vol. xiv. page 708, and vol. xv. page 123.

heat deepens the color, and boiling with acid changes it to green-blue.

As additional characters which may assist in the detection of acetone, Le Nobel cites its reaction with tincture of iodine and ammonia; one-ten-thousandth of a milligramme of acetone then betrays its presence by the production of iodoform, while under the same circumstances alcohol gives no results. Also, the one-hundredth of a milligramme of acetone may be detected by its power of dissolving mercuric oxide. In this test a solution of mercuric chloride is precipitated by the addition of alcoholic potassium hydrate, and then agitated with the liquid supposed to contain acetone; the mixture is filtered, and the filtrate tested for mercury with ammonium sulphide. Lead hydrate also dissolves in acetone in the absence of alkaline hydrates.

Ethyldiacetic acid gives no iodoform reaction, and behaves somewhat differently from acetone when subjected to Weyl's reaction. Mercuric oxide and lead hydrate are dissolved by ethyldiacetic acid. This acid distils without decomposition when its solution is boiled, and it is undecomposed by organic and dilute mineral acids; it is, however, decomposed by strong mineral acids, especially by the aid of heat.

Aldehyde simulates creatinine in Weyl's reaction still more closely than either of the other two bodies mentioned.

Le Nobel criticises Von Jaksch's method for the estimation of acetone, and considers that, even if acetone be constantly present in cases of acetonuria, its quantity is quite insignificant, but appears to increase after the ingestion of alcohol or sugar.

Normal urine contains still another substance which gives the iodoform reaction. In fever there is often decided acetonuria, so that acetone can be recognized in the distillate from five hundred cubic centimetres of urine; but the production of acetone is not dependent on the fever intensity, being greater when there is a large amount of indican present. Diabetic coma does not appear to depend on acetonuria, and an increased proportion of acetone in the urine is not to be regarded as an alarming symptom. The red-brown reaction of urine with ferric chloride does not depend on the presence of ethyldiacetic acid, and Le Nobel considers it a

doubtful question whether it is always due to acetylacetic acid, as held by Von Jaksch.

It may also be mentioned that Le Nobel claims to have found ethyl alcohol and acetone (possibly ethyl-acetic acid) in the breath exhaled by diabetics.

E. Salkowski reclaims priority for Legal's and Le Nobel's modification of Weyl's reaction, and states that Le Nobel is wrong in asserting that the creatinine reaction does not give a blue color on boiling in the presence of acetic acid.

FOR THE ESTIMATION OF NITROGEN in urine and excrements, W. Camerer places the substance to be analyzed in a glass tube sealed with paraffin. This is then inserted in a long combustion-tube with soda lime, and the analysis is conducted in the usual manner. Camerer finds that the nitrogen in *faeces* must be estimated in fresh material: drying at a temperature of 100° – 105° C. occasioned a loss of 0.109 grammes of nitrogen for 100 grammes of fresh *faeces*, or 8.2 per cent. of the total nitrogen present. The estimation of nitrogen in urine by the hypobromite method gave, in comparison with a similar estimation by combustion with soda lime, a loss of 10.9 per cent.

THE RELATION BETWEEN THE ELIMINATION OF PHOSPHORIC ACID AND THAT OF NITROGEN IN THE URINE DURING ALIMENTATION BY BRAIN-SUBSTANCE has been investigated by G. Politis. The proportion of phosphorus in brain-tissue is known to be higher than in other animal tissues, and the author has found in fresh ox-brain 0.81 per cent. of P_2O_5 for 1.69 to 1.78 per cent. of nitrogen. In one experiment a dog was fed on bones for one day, then for three days exclusively on ox-brains, and on the fifth day again on bones. The ratio between the P_2O_5 and nitrogen eliminated in the urine on those days was found to be 1:5.02, 3.2, 2.2, 2.0, 2.2, and the curves representing the daily excretion of phosphorus and of nitrogen were sensibly parallel. During an exclusive flesh-diet Feder found a much more rapid elimination of phosphoric acid in the urine, and Politis explains this by the fact that the phosphorus in flesh exists in the form of phosphates, while that in brain-substance is in the form of complex ethers, such as lecithin. The latter is absorbed with difficulty, and during nutrition with brain-substance the author has found it in the

fæces, together with a very small quantity of glycerin-phosphoric acid. Bokay found no lecithin in the fæces during a diet of yolk of egg. In another experiment of Politis a dog was fed exclusively on flesh, and the ratio of the phosphoric anhydride to the nitrogen in the urine was found to be 1:6.7; when the diet was fifty parts of brain-substance to one of flesh, both nitrogen and phosphoric acid increased in the urine without the proportion between them sensibly varying: hence the before-observed variations in this proportion cannot with any probability be attributed to the influence of the ingested brain-substance.

It is to be remembered that Gamgee, Paton, Cazeneuve, and Lépine could detect no increase in the elimination of phosphoric acid in the urine after mental work. The diminution in the excretion of phosphoric acid observed by B. Schulze after the internal administration of potassium bromide is due, according to Politis, to a retention of phosphoric acid in the blood, this retention depending on a double decomposition between the potassium bromide and the sodium phosphate.

THE INFLUENCE OF WATER ON THE ESTIMATION OF UREA BY LIEBIG'S METHOD.—Luzzatto has again studied the effect of dilution on the final reaction in Liebig's method, which, it is well known, requires for dilute urea solutions a larger quantity of the mercuric nitrate solution, and for concentrated solutions a smaller quantity than normal. As Pflüger had already recommended, Luzzatto, in a recent communication, counsels that the final reaction with sodium carbonate should be made in the entire bulk of liquid, especially when this bulk is small.

KAIRIN (oxyhydroethylchinolin), according to Petri and Lehmann, occurs as a salt of sulphuric acid in the urine in phthisis. The compound is not destroyed by the ammoniacal fermentation of urine, but is decomposed by long boiling with hydrochloric acid. In dilute acetic acid solution it gives a fuchsin-red color on the addition of a little chlorinated lime solution, but the color appears only in about half an hour. The spectrum of the solution shows an absorption band between D $\frac{1}{2}$ E and F. Kairin itself gives, under these circumstances, a different red, which soon changes to dark-brown with a faint violet reflection.

REMOVAL OF UTERUS AND OVARIES BY ABDOMINAL SECTION FOR FIBRO-MYOMATA—DEATH ON TWELFTH DAY, FROM SEPTICÆMIA.

Read before the Philadelphia County Medical Society, December 17, 1884.

BY J. M. BARTON, M.D.

MRS. K., æt. 31 years, was brought to me last September by Dr. M. B. Dwight, of Jersey Shore, Pennsylvania. She was very pale, the face and lips being entirely colorless: indeed, the appearance of the patient suggested the presence of malignant disease. She had been losing more blood at her menstrual periods than she should, for several years, and for several months she had a daily loss, frequently in quite large amounts. She was losing strength rapidly, and had to be carried to her room on her arrival in this city.

On examination, a large, hard, smooth, and freely-movable tumor, evidently the uterus, extending three inches above the umbilicus, was found; by the sound, Dr. Dwight's diagnosis of a submucous fibroid with extensive uterine attachments was readily confirmed.

As the uterus was entirely out of the pelvis, as the attachments of the tumor occupied more than three-fourths of its entire circumference, and as the remaining uterine wall was much thinned, removal by the vagina was plainly impossible. I advised extirpation of the ovaries, if accessible, or of the entire uterus, if, on exploration, it appeared preferable.

The patient returned to her home, as she preferred to have the operation performed there, to which I agreed, as I considered the mountain air much more favorable for operation and after-treatment than the wards of a general hospital.

On September 30 I visited Jersey Shore and removed the growth, assisted by Drs. Dwight and Cline, of that place, Drs. Detweiler and Youngman, of Williamsport, Dr. Armstrong, of Lock Haven, and Dr. Orville Horwitz, of Philadelphia.

Thorough antiseptic precautions were taken: the hands of the operator and assistants were washed in carbolic acid solution; all the instruments and ligatures were immersed in a similar mixture; the sponges were washed in a warm solution of the same acid; the abdomen of the patient was washed first with turpentine,

then with soap and water, and lastly with carbolic acid solution.

I made a vertical incision through the abdominal wall, midway between the umbilicus and pubes, about two and a half inches long, through which I readily drew the right ovary; the left, however, could not be reached. Finding the uterus free from adhesions, we decided to remove it. I increased the incision until it ran nearly from ensiform cartilage to pubes, carefully checking all hemorrhage by forceps and catgut ligatures before opening the peritoneum; the uterus was readily lifted from its bed and placed upright; the left broad ligament was attached posteriorly and the left ovary lay against the spine, showing that it could not have been reached through the original incision. The intestines were held away from the uterus and supported by large flat sponges wrung out of warm carbolic acid solution. The broad ligaments were tied in sections with carbolized silk, and Thomas's large clamp placed upon the neck of the uterus.

Surrounding the parts with sponges to prevent blood from entering the peritoneal cavity, into which so far none had flowed, the uterus and both ovaries were rapidly removed. There being but little tension on the pedicle, we decided to treat it outside.

The stump was trimmed so as to leave but little projecting above the clamp; it was transfixed by a large pin, and was seared on its cut surface by the actual cautery; the wound was closed by the interrupted suture introduced from within in the usual manner. It was not found necessary to make the "toilet of the peritoneum," as, thanks to the care of my assistants, no blood had been allowed to enter that cavity, and, indeed, but little was lost during the entire operation.

The uterus was seven inches in diameter and nearly a sphere; it was occupied by a single fibro-myoma, which was attached to the entire uterine walls, with the exception of a narrow channel about two inches wide, running from neck to fundus. On section, it presented the usual appearance of such growths, except in its centre, where it appeared to have undergone sarcomatous degeneration. This suspicion was subsequently confirmed on microscopical examination.

The after-treatment of the case was in the hands of Dr. Dwight, from whose

very complete notes I take the following points:

On the evening of the operation, the temperature rose to 102.5° , the pulse to 120; under opium suppositories and occasional hypodermics of morphia the patient was quite comfortable; she took nothing whatever into the stomach except small quantities of hot water, and had no vomiting.

On the second day the thermometer rose to 104° , the maximum temperature observed.

On the third day she took some barley-water, and in the evening some beef-tea, the general condition being improved and improving still more on the fourth day.

On the fifth day (October 4) the bowels were moved by an enema; opium stopped; beef-tea, brandy, and warm milk were taken freely; the patient was quite comfortable.

On the sixth day (October 5) patient rested well during the night; takes warm milk every three hours; pulse stronger, 120; temperature, 101° . Patient looks quite bright; natural movement of the bowels.

The notes of the seventh, eighth, and ninth days are almost identical with the last one read, except that the stitches were removed, and on the ninth day there was considerable pus coming from the wound, and some swelling of the right parotid gland.

On the tenth day the swelling of the parotid gland increased, there were evidences of systemic poisoning, and on the evening of the eleventh day she died.

At the autopsy, there was no inflammation of intestine; there was no pus found in lungs, liver, or kidneys, though there was some in the abdominal cavity. This had probably entered a day or two before her death, and from it arose the blood-poisoning, which proved fatal.

INDIGO-FORMING SUBSTANCES IN THE URINE.—J. Hoppe-Seyler (*Zeitschrift für Physiol. Chem.*, Band viii.) obtained one gramme of crystallized indican from twenty-five litres of normal dogs' urine, in which it exists in sufficient quantity to be detected by Jaffe's reaction. To determine if indican undergo any change in the organism, 2.7 grammes were injected into an animal, but in the urine indican only was found. No increase was caused in its elimination by the injection of orthonitrocinnaemic acid, or of orthonitrobenzaldehyde alone, or together with acetone.—*London Medical Record.*

NOTES OF HOSPITAL PRACTICE.

PENNSYLVANIA HOSPITAL.

CLINICAL SERVICE OF THOS. G. MORTON, M.D.,

One of the Attending Surgeons to the Hospital.

*Lecture delivered November 29, 1884.**A CASE OF COMPOUND, COMPLICATED, COMMUNICATED FRACTURE OF THE HUMERUS.*

GENTLEMEN,—The first case that I will present this morning is one of a sailor, 54 years of age, who has sustained a compound complicated fracture of the lower third of the humerus by a fall on board ship two weeks ago. It is compound, because the fractured ends of the bone can be felt in the open wound; complicated, because the injury extends into the joint. A compound fracture is not necessarily a complicated fracture, even when, as in this case, it is at the same time comminuted. A fracture becomes complicated in a surgical sense when it is attended by injury of important structures, such as large blood-vessels or nerve-trunks, or when it extends into a joint. The fracture may, therefore, be complicated without being compound or comminuted.

Such injuries as the one before us are generally grave in proportion to their extent, or to the involvement of the joint surface. Immediately following such an injury, the synovial membrane lining the joint (which in health secretes simply enough fluid to lubricate the articulation) becomes inflamed, and effusion follows. The injury, frequently, is also sufficiently great to cause contusion of some adjacent structures, followed by more or less inflammation. The result is inflammation of the synovial membrane, or synovitis, and changes in its structure, which, with the fracture, leads to permanent alteration in the joint. Where the force has been sufficiently great to produce a compound fracture, there is usually severe inflammation. In either case the changes occurring in the synovial membrane are the same as those in hip-joint disease, or white swelling, the inflammation in one case being traumatic, in the other the result of constitutional disease.

Now, the chief danger from the accident which this elbow-joint has sustained does not consist simply in the fracture of the bone, but in the amount of injury

to which adjacent structures have been subjected. If it were simply a fracture of the bone, we might have repair without marked degree of inflammation; but, where there is so much laceration, repair must take place by inflammation, which often results in abscess. This is what occurred here. I was obliged to make free incisions on the back of the arm to give vent to serum and pus. If you notice here, you will see a slight blush of inflammation upon the radial aspect of the arm, where there is another subcutaneous collection, which is either an abscess or a mass of broken-down and partially-decomposed blood.

In the prognosis of such injuries two things must especially be kept in mind,—the nature and the location of the lesion. The prognosis also varies with the age. A child with a compound fracture of the elbow—say a boy of 12 years of age—will give much better results from the treatment than a man of 50. One reason is that the reparative processes are more active in a child, on account of its growing condition; another is that these fractures in adult life usually occur in laboring-men who are given to the excessive use of alcohol, and their vital powers are impaired by their mode of life. In a child we are often able to seal up the surface-wound and to convert the case at once into one of simple fracture; this can only rarely be done successfully with adults. Every year of life reduces the chances of recovery with subsequent usefulness of the joint.

In cases of fracture involving a joint the prognosis varies with the importance of the joint and its size. It is worse in a hinge-joint like the elbow. You know that a hinge that is clogged by paint or any foreign substance cannot work smoothly. So in this joint, as a result of the fracture and changes in the synovial membrane, there will be more or less ankylosis, depending upon the plastic exudation and the inflammatory changes in the bones and membranes.

The effects of such injury may be inferred from this dry specimen from the museum of the hospital, illustrating a compound fracture of the lower end of the humerus extending through the condyles and involving the joint; the shaft above is broken in a transverse direction.

As regards the diagnosis, the symptoms

of such fractures are generally so well marked that if the surgeon see the case immediately after the injury there can be no mistake in the opinion he forms of the fracture; seen later, there may be so much swelling as to interfere with the diagnosis. In all cases where the nature of the case is not plain, it is better to give ether and carefully examine the injury. The joint may be invaded by a fracture from any direction; very often there is also luxation, due to the separation of the condyles. The bones of the forearm may be forced up so violently as to separate the fragments of the humerus, causing a luxation backwards. When the fracture is compound you can readily detect the fracture by inserting the finger if the wound be a large one.

Some of the most difficult of these cases of injury around the elbow-joint are those of fractures involving the head of the radius; sometimes it is almost impossible, even with the aid of ether, to make out the exact line of separation of the fragments.

With reference to treatment I will say a few words. In the first place, it is always proper to try to convert a compound into the simple variety of fracture by hermetically sealing it at once with lint and the compound tincture of benzoin, and covering the wounded part with a solution of laudanum and lead-water. It also is advisable to elevate the limb, and to give the patient anodynes. If you cannot convert it into a simple fracture, you must prepare yourself for what is likely to occur after compound fracture,—cellulitis and abscess. This is what has occurred in the patient before you. Sometimes the appearances are those of erysipelas, which you should also be prepared for. Where abscess forms, it must be evacuated early, in order to relieve the part from the irritation of the purulent material.

It is always well to bear well in mind the anatomical relations in making your incisions. You know that the principal blood-vessels and nerve-trunks lie upon the inner and anterior aspect of the limb, placed there because they are in this position less liable to injury. There is nothing, therefore, to make you fear long or deep incisions in the outer aspect of the arm. Here I make an incision at least five inches upon the arm and forearm, which gives free vent to the pus. This abscess seems to have

been consequent upon injury received by the soft parts at the time of the injury; I cannot perceive any connection between the abscess and the fractured joint with my finger. It is an excellent plan after etherizing the patient to use the finger as a probe, so as to establish more accurately the limits of an injury.

When he was admitted there was a large amount of swelling, and the patient refused at that time to take ether to permit a careful examination. I now find a large portion of the upper fragment of the humerus denuded of periosteum. I will extend the wound through the triceps muscle, in order to make the bone project. Having exposed it, I now remove about three inches of it with the saw, and pack the wound with carbolized lint, and apply a posterior right-angled splint. It is a case in which amputation will probably be necessary.

[At the next clinic amputation of the arm at the middle was performed. The patient afterwards made a good recovery. —REP.]

EXTENSIVE KELOID, PRODUCING GREAT DISFIGUREMENT OF THE FACE AND NECK.

This colored man, 32 years of age, was engaged in the working of a cotton-press eighteen months ago, when he was so unfortunate as to be severely burned in his hands and face and upper portion of



his breast and shoulders by an explosion. The burns healed well, but, as you see, the cicatrices have taken on keloid growth, which has involved in a mass of new growth the neck, both auricles, and the sides of his face, producing great disfigure-

ment, interfering with movement of the jaw and with hearing. We have applied collodion to the growth, and it has apparently led to some contraction, but probably no more than any dressing would which exercised pressure. We would probably have had just as good results from an ordinary bandage.

I propose this morning to amputate a portion of this tissue, in the hope that a healthy scar may be obtained, such as you see in his hands. I will remove the mass surrounding the right side of the face, as the growth here interferes with his hearing. I can save part of the pinna of the ear.

[This operation was performed without much bleeding. Two weeks later the wound had nearly closed, and the patient was willing to have the other ear operated upon. This was performed Saturday, December 20. The growth, which covered both ears, the sides of the face, and the neck, was successfully excised.—REP.]

TRANSLATIONS.

TREATMENT OF ACUTE PNEUMONIA BY BATHS.—If pneumonia be caused by the development in the human body of a specific microbe, as Dr. Chaumier (*Congrès de Blois*) maintained in a former communication, he insists that more account should be taken of this important etiological discovery as a basis for treatment. We should not, he says, add to the poison in the system another which is able to cause death as well as the first; and he unsparingly condemns venesection and blisters on account of their debilitating effects. Statistics prove that pneumonia abandoned to itself pursues a more favorable course than when treated by emetics, bleeding, and blisters. Although the disease cannot be cured, much can be done to relieve the symptoms, especially by cold baths. Each bath, at a temperature of 28° to 32° Cent. (82° to 90° Fahr.), was at once followed, in his experience, by an amelioration of the symptoms, lasting two or three hours. Two or three such baths were given daily. The author gives the following conclusions, based upon fifteen cases (fourteen were children), of which all recovered:

1. Cold baths do not present any danger in pneumonia at any period at which they may be employed.

2. They reduce the temperature and the

frequency of the pulse, lead to the disappearance of the *bruits de souffle* caused by the fever; they diminish also the dyspnoea considerably; they relieve the thirst and permit a more abundant alimentation.

3. Having only used the bath with those who recovered without other treatment, M. Chaumier maintains a sage reserve as to the results to be expected from the treatment: he does not assert that these baths diminish the mortality, although everything leads him to believe that they do.

4. In adults, in order to obtain a durable amelioration, a bath should be given every two or three hours.—*Revue de Thérapeutique Médico-Chirurgicale*.

[Baths at the temperature indicated would not usually be considered as cold baths, but for pneumonia they are certainly preferable to cold baths as generally understood.—ED. P. M. T.]

INCOMPATIBILITY OF QUININE AND IODIDE OF POTASSIUM.—M. Rabuteau, having seen gastric disorder, anxiety, nervousness, and general malaise follow the simultaneous administration of iodide of potassium and sulphate of quinine, attributes the symptoms to the chemical reaction, with local irritation of the stomach by the nascent iodine. He also warns against the administration of quinine to a menstruating patient, as serious and even fatal symptoms may ensue.—*Gazette des Hôpitaux*.

MORBUS MACULOSIS WEILHOFII.—Dr. Th. Hryntschak, of Vienna, publishes in the *Arch. f. Kinderheilk.* some experiments carried on with a view of deciding the question as to the contagiousness of morbus maculosis Weilhofii. He was not able to verify the results obtained by Petrone, inasmuch as the rabbits which he experimented upon did not suffer from the disease either when vaccinated directly or with the culture-fluids.

OLEUM GAULTHERIÆ IN ACUTE RHEUMATISM.—At the clinic of Professor Rossbach oil of gaultheria has been employed in a large series of cases of acute articular rheumatism, followed by equally prompt and striking results as had been obtained previously from the salicylate of soda. It is recommended to give it in capsule, after eating, on account of its disturbing effect upon digestion.—*Correspondenz-Blatt der Allgem. Aerzt. Ver. von Thüringen*.

PHILADELPHIA
MEDICAL TIMES.

PHILADELPHIA, JANUARY 24, 1885.

EDITORIAL.

THE COAGULATION OF THE
BLOOD.

AN able and interesting review of the various theories which have been prominently upheld of late years concerning the coagulation of the blood is contained in a paper by Frederick P. Henry in the *Archives of Medicine*.^{*} After premising the statement that coagulation is always a pathological process and never physiological, and referring to the extremely apt phrase of Hayem, that it is a kind of rigor mortis, or "cadaveric rigidity" of the blood, the writer points out that the act of coagulation furnishes most important data for the study of the physiology of the blood, being a sort of spontaneous analysis the thoroughness of which may be aided by external conditions.

Dr. Andrew Buchanan, of Glasgow, in 1831, after an ingenious investigation, concluded that coagulation was dependent upon the presence of the white blood-corpuscles, the fibrin pre-existing in a state of solution in serous fluids and liquor sanguinis without any spontaneous tendency to coagulate; upon these fluids the white cells acted as a sort of ferment, producing an effect like that of rennet upon milk. Dr. Alexander Schmidt, in 1861, claimed that this action of the white blood-cells was due to their destruction and disintegration, which liberated serum-globulin: this he termed the fibrino-plastic substance. The coagulating property of transudates he found to reside in a substance which he identified also as belonging to the class of globulins, to which he gave the name of fibrinogen. Fibrin he supposed was

formed by the combination of these two substances. This view he afterwards modified by invoking the aid of a third agent or factor in the coagulation, which he called the fibrin-ferment.

According to Schmidt's theory, then, blood-coagulation is due to the union of the substances fibrinogen and serum-globulin, under the influence of a ferment,—the first-named being the only one pre-existing in the blood, the others being formed after the blood has been withdrawn from the vessels. Hammarsten has more recently broached the view that the serum-globulin is an unnecessary factor in the formation of fibrin.

In contrast with those who consider fibrin to be caused by the union of soluble substances in the liquor sanguinis, we have the observations of Wooldridge, Norris, Hayem, and Bizzozero,[†] who attribute its formation, in great part at least, to a transformation of the morphological elements of the blood. The first-named holds the opinion that the plasma changes the leucocytes directly into fibrin, but at the same time a substance is liberated from the cells (fibrin-ferment) which converts fibrinogen also into fibrin. The remaining authorities mentioned agree in associating the formation of fibrin with changes in certain morphological elements of the blood, Norris attributing it to changes in what he terms the invisible corpuscle or advanced lymph-disk, the two others to similar changes in bodies termed by Hayem *hématoblasts*, from their supposed share in the formation of the red blood-corpuscle, and by Bizzozero *blutplättchen*. What the exact rôle played by these colorless corpuscles, or blood-plates, may be, is not yet determined, but it has not been established nor even positively claimed that they are the sole agents in the production of fibrin, as the original authorities concede that the white corpuscles also may be concerned in fibrin-formation.

* Vol. xii. No. 3, December, 1884.

† Quoted by Dr. Henry.

In the opinion of Dr. Henry, a blood-clot is the result of two distinct processes,—coagulation proper and fibrin-formation,—the one chemical, the other morphological. Coagulation occurs in milk under the influence of a ferment, but there is no fibrin-formation. "The same process takes place in blood under the influence of a ferment derived from the white corpuscles, and perhaps also from the blood-plates, while at the same time, and in addition, there is formed a fibrinous reticulum which is a direct metamorphosis of the same elements."

It is possible that there may be still a third, and as yet disregarded, factor in the formation of a blood-clot,—*i.e.*, with the chemical and morphological may be conjoined a vital process. The view of the blood which regards it as a living tissue, endowed with physiological energy manifested normally upon condition of its incessant motion in healthy vessels, may also see in fibrin-formation an abnormal and aberrant expression of nutritive force. The term "cadaveric rigidity" of Hayem is certainly very suggestive.

COMMA-BACILLI AND CHOLERA.

THE positive claim of Dr. Koch that the comma-bacillus is the *materies morbi* of epidemic cholera, made in his report to the German government, has not yet been implicitly acknowledged by other competent observers, who have with great care gone over the ground traversed by the great mycologist. Drs. Klein and Heneage Gibbes, in their preliminary report of the result of their investigations carried on in Bombay and Calcutta, declare without any circumlocution that "the statement of Koch that 'comma-bacilli' are present only in the intestines of persons suffering from or dead of cholera is not in accordance with the facts, since 'comma-bacilli' occur also in other diseases of the intestines,—*e.g.*, epidemic diarrhoea, dysentery, and in intestinal

catarrh associated with phthisis." Other statements meet with a similar flat denial, and all attempts at inoculating animals with cholera-fluids or bacillus-cultures completely failed. The bacilli in question were cultivated, and it was found by Dr. Klein that they did not behave differently from ordinary putrefactive organisms.

In replying to similar criticisms upon the comma-bacillus recently, Dr. Koch remarked that he remembered that "they made some such remarks about the bacillus tuberculosis two years ago." This is a coincidence which is not without significance.

COLLECTIVE INVESTIGATION.

WITHIN the last week or two there have been distributed to the members of the State Medical Society of Pennsylvania circulars, accompanied by three blank forms, which have been sent out by the Committee on the Collective Investigation of Disease appointed at the last meeting of the State Society. The cards are of different colors, as adopted by the British Association committee,—buff, green, and red,—and are respectively for pneumonia, chorea, and acute rheumatism. The questions have been so arranged as to be answered with the least expenditure of energy and time on the part of the observer, and it is hoped that every member of the Society will feel it incumbent upon him to aid the committee in its work, and will avail himself of the opportunity to report a case of each disease, in order that a fair trial may be made of this method of collective investigation, which is affording such satisfactory results in England. The cards, after being filled up, are to be returned to the address of the chairman, Dr. James Tyson, in Philadelphia, not later than the first day of April, 1885, but the committee requests that they be returned earlier if possible, in order that the labor of their collation may be rendered less arduous.

STATE BOARD OF HEALTH.

A CONFERENCE meeting of delegates from the various local Boards of Health of the State was held at Harrisburg January 13, to take measures looking towards the establishment of a State Board of Health for Pennsylvania. A bill has been carefully prepared, and it is expected that a determined effort will be made to have it passed at the present session of the Legislature. The recommendation contained in the annual message of the Governor has given encouragement to those who have for so long had the matter in charge, and there now seem to be better prospects for such a bill than at any previous session. The Board is to consist of six members, not necessarily physicians, but the Secretary must be a sanitary expert, who shall receive a salary of two thousand dollars a year. The entire appropriation for the Board is limited by the proposed bill to ten thousand dollars.

The members in attendance upon the conference organized a State Sanitary Association, Dr. Green, of Pittsburg, President, which will act provisionally as a voluntary board of health until the bill is passed.

NOTES FROM SPECIAL CORRESPONDENTS.

NEW YORK.

PROFESSOR AUSTIN FLINT ON DYSPESPIA.

AT the meeting of the New York State Medical Association, November 18, 1884, Dr. Flint read a paper, entitled "Remarks on the Dietetic Treatment of Dyspepsia," in which he gave expression to views which would lead us to suppose that even as great a mind as his may be influenced to lay aside its judicial qualities and take the ground of the extremist. It would seem that, having seen a number of cases of dyspepsia in which the patients were benefited by abandoning their self-regulated diet, the author had allowed himself to conclude erroneously that appetite is an infallible guide in the proper selection of food, both for the well and for the functionally diseased. At the beginning of his paper Dr. Flint makes a, seemingly, very just distinction between indigestion and

dyspepsia: "The name (dyspepsia) signifies difficulty of digestion. Now, digestion may be difficult, and attended by more or less suffering and disturbance of the nervous system, the digestive function, nevertheless, being duly and completely performed." But farther on, in speaking of the symptomatology of dyspepsia, he loses sight of this distinction, and applies his remarks to a train of symptoms in which we may justly suppose that indigestion plays a more or less important rôle; and for this reason his paper is open to a greater degree of criticism. Indeed, it would be difficult to make a strictly clinical distinction between indigestion and simple dyspepsia (dyspepsia without indigestion). Dr. Flint evidently recognizes this difficulty when, in speaking of dyspepsia, he says that in many instances there is coexisting anæmia, although he adds, "The dyspepsia leads to impoverishment of the blood, often because alimentation is reduced below the needs of nutrition, and this condition tends in no small degree to increase the dyspepsia and render it persistent."

The author's instructions with regard to the diet of persons suffering from dyspepsia seem to us to be particularly objectionable, because, coming from a gentleman of so high authority, they are liable to lead many of his professional brethren and the laity to errors of teaching and living. He says, "Suffice it to say that the maxims which have been enumerated as causative of dyspepsia are to be reversed in pursuing the plan of treatment which it is the purpose of this paper to submit. The instructions, abbreviated, will then be as follows:

"Do not adopt the rule of eating at stated periods,—twice or thrice daily. Be governed in this respect by appetite; eat whenever there is a desire for food. Eat in the evening, or at bedtime, if food is desired. Insomnia is often attributable to hunger. In the choice of articles of diet, be distrustful of past personal experience, and consider it to be a trustworthy rule that those articles will be most likely to be digested without inconvenience which are most acceptable to the palate. As far as practicable, let the articles be made acceptable by good cooking. As a rule, the better articles of food are cooked the greater the comfort during digestion. Never leave the table with an unsatisfied appetite. Be in no haste to suppose that you are separated from the rest of mankind by dietetic idiosyncrasies, and be distrustful of the dogma that another man's meat is a poison to you. Do not undertake to estimate the amount of food which you take. In this respect different persons differ very widely, and there is no fixed standard of quantity which is not to be exceeded. Take animal and vegetable articles of diet in relative proportions as indicated by instinct. In the quantity of drink follow na-

ture's indication,—namely, thirst. Experience shows abundantly that, with a view to comfortable digestion, there need be no restriction in the ingestion of fluids."

We cannot help but feel that in laying down these maxims Professor Flint has reversed those rules which common sense would dictate, and which general experience has proven to be true, and that he has been led to accept the reverse of the truth by having observed certain cases in which persons desiring to follow those rules had failed to do so in fact, and their symptoms had grown worse therefor. At the same time he seems to have overlooked the fact that dyspepsia, in the sense in which he employs the term, may be due to different causes. The affirmative rules to be observed in the treatment of the dyspeptic, the negative of which he recommends, doubtless grew out of the fact that there are different forms or causes of dyspepsia, and it is more than probable that Dr. Flint, in his large experience, has met with examples of these different forms. One person has dyspepsia because his appetite has led him to overeat, in evidence of which we may quote the author of the paper, who says, "The appetite may be more or less impaired, but it is often not diminished, and not infrequently it is increased." Now, if the appetite be increased we may justly suppose that some persons would be ignorant or morally weak enough to cater to their natural inclination and gormandize; and if this be true, the advice to dyspeptics indiscriminately to eat at no stated periods, and to never leave the table with an unsatisfied appetite, could not do otherwise, in many cases, than to work evil. These persons might be able to digest a normal quantity of good food, taken at proper times, without its giving rise to any dyspeptic symptoms or ill health, and their dyspepsia is really due to an excess of food which they are not capable of digesting (therefore due indirectly to indigestion). Were they able to digest all they ate they would fall among those of whom Dr. Flint speaks as follows: "Finally, let us learn a practical lesson from our observations of the class who 'live to eat,'—the *gourmet* and the *gourmand*,—they 'who fare sumptuously every day.' Dyspeptics are not common among this class." But before reaching the age at which they can be classed among those "who fare sumptuously every day" their dyspeptic symptoms become so aggravated that they are obliged to cease "living to eat." The writer once suffered from dyspepsia, which finally led to broken health, brought on when a boy at a time when his appetite was ravenous, and was allowed unregulated freedom, at all hours of the day and night, in the midst of an abundance of tame and wild fruit, vegetables, and a well-supplied pantry. His symptoms of dyspepsia disappeared and health returned only after

the rules were obeyed which Professor Flint advises us to disregard.

Again, the statement, "Consider it to be a trustworthy rule that those articles will be most likely to be digested without inconvenience which are most acceptable to the palate," seems to us to be of doubtful propriety. It is just as likely, if one is unable to digest a given kind of food with convenience, that it will be among those articles for which he has an appetite as otherwise. Dyspepsia, in the sense in which it is used by the author of the paper, may reasonably be supposed to be due to a peculiar affection of certain parts of the nervous system, and experience proves that the appetite may demand certain articles of food which, when ingested, affect that part of the nervous system in a manner to produce dyspeptic symptoms.

"Experience shows abundantly that, with a view to comfortable digestion, there need be no restriction in the ingestion of fluids." The author does not say what kind of fluids, but, even if he meant only water and milk, there are, doubtless, many among both physicians and the laity who would reject the statement as too sweeping, and could bring forward abundant evidence showing that persons had injured their powers of digestion by indiscreet drinking of fluids during meals and in the intervals. Several physicians in this city attribute the weak and "run-down" condition, in nearly all cases among their female dispensary patients, to the habit of drinking an excess of tea, or perhaps of coffee. The author seems not to have considered the fact that the appetite for food and drink depends much upon habit, and that many people acquire habits of eating to excess particular articles of diet, of drinking perhaps three or more cups of black coffee with each meal or a large quantity of beer, and of eating at hours when their business will least favor digestion.

Objections might also be made to certain other of his maxims, but we will close by saying it is difficult to avoid the conclusion that Professor Flint, in his desire to correct an apparent tendency on the part of that class of patients of an active mental development to go to a certain extreme, has allowed himself to fall into the error of advising all classes of dyspeptics to go to the opposite and worse extreme. May not this error be due to his practice being largely among the thinking class of people and less among the laboring and uneducated? We should think this probable, inasmuch as he says dyspeptic patients do not belong among hard-working laborers or mechanics. From our observation of men, we have been led to believe that there are many cases of dyspepsia among this class of persons, and here we think the rules which Professor Flint advises all dyspeptics to disregard would be

especially commendable, whereas in the class in which he is most commonly consulted it might perhaps be better advice if they were told to labor more with their muscles in healthful employment, to read and study less, and to follow the promptings of their vegetative system,—appetite, sleep, etc.

One paragraph, including this sentence, "There are various incidental points of inquiry, and certain qualifications of statements, which a full consideration of the subject would embrace," gives the impression that the author does not differ so radically from us in his views of the subject as the general tone of the paper would imply; and, if this be true, we regret that he did not take more time and qualify the statements which may, as they now stand, prove to some a stumbling-block in the way of recovery from dyspeptic, and also general digestive, disorders.

PROFESSIONAL NOTES.

The Medical Department of the University of the City of New York has sustained a serious loss in the death of William Darling, M.D., LL.D., F.R.C.S., for many years Professor of Anatomy. Dr. Darling was said to be given to eccentricities, and among others was one which students of medicine would be glad to recognize in all teachers,—viz., that of beginning and ending his lecture on the stroke of the clock. It has been reported that none of Professor Darling's relatives survive him; but a physician-friend told me that he is acquainted with one of his brothers, who resides at or near Burlington, Iowa.

It has been over two weeks now since Dr. Richard C. Brandeis disappeared, and up to the time of this writing no information has been received of his whereabouts. Dr. Brandeis was a graduate of the College of Physicians and Surgeons (1871), and was one of the Professors of Diseases of the Nose, Throat, and Ear at the New York Polyclinic.

About a week ago Dr. Rudolph Tauszky shot his wife and then himself, the ball in both cases striking the head, but probably not inflicting wounds which will cause death. It is thought that he was temporarily insane when he committed the deed. Dr. Tauszky has been a liberal contributor to medical literature, had made more or less a specialty of the diseases of women, and had recently testified as a medical expert in several cases of insanity.

A number of cases of typhus fever have developed in a West Thirty-second Street tenement-house, and the health authorities are having a little trouble in stamping out the endemic. One hundred and fifty-seven cases of measles were reported by the Board of Health for the week ending January 6, fifty-nine of diphtheria, fifty-eight of scarlet fever, and thirteen of typhoid fever.

The Directors of the New York Dispensary hereafter propose to appoint three physicians

who shall have passed a competitive examination, paying them a salary of eight hundred dollars annually for services of three or four hours' duration at the daily dispensary. Would not this be a good plan for diminishing the unnecessarily large number of our free dispensaries and so-called hospitals? But so long as the city physician remains as ambitious as he at present appears to be to have his name connected with some public institution, this change for the better will probably not be brought about except through the co-operation and instrumentality of directors of hospitals and dispensaries. The question might suggest itself to the curious whether in any given case the change was not due to a supposition that better services would be rendered the sick poor by a few physicians who received the moral support of a pecuniary consideration than by a larger number, who, inasmuch as their services were entirely uncompensated, might feel themselves justified in trying to advance their medical skill and reputation by practices which would be rejected by one whose eye was single to the good of his patients.

R. C. S.

PROCEEDINGS OF SOCIETIES.

PHILADELPHIA COUNTY MEDICAL SOCIETY.

AT a clinico-pathological meeting of the Society, held at the Hall of the College of Physicians, December 17, 1884, the President, Dr. W. M. Welch, in the chair, papers were read by Drs. J. M. Barton (see page 316), Dr. W. C. Hollopeter (see page 304), and others.

DISCUSSION ON INVERSE TEMPERATURE IN TYPHOID FEVER.*

Dr. J. C. Wilson, in opening the discussion, said, "The paper of Dr. Hollopeter is very interesting and suggestive. It touches upon or suggests almost every important clinical problem relating to this disease. Its free discussion, therefore, would be impossible in one, or indeed in many evenings. Two or three points call for special consideration.

"First, the typical temperature-curve and its variations. The well-known course laid down by Wunderlich as a standard is met with perhaps less frequently than strongly-marked variations. Nevertheless, a knowledge of that course is the key to the understanding of the variations from it. Wunderlich, after describing the course of the first four or five days, lays down the law that every case which does not conform to this temperature-type is not typhoid fever. This dictum has been the source of manifold er-

* Reported by G. Betton Massey, M.D.

rors in diagnosis. No more fallacious statement could be made. Cases of undoubted typhoid fever constantly occur in which the range of temperature fails to conform to the typical curve, not only in the beginning of the attack, but throughout the whole course of the disease. In fact, cases sometimes occur, the nature of which is fully established by the complexus of symptoms and the existence of local epidemics, in which the temperature remains throughout even *sub-febrile*. The singularly labile temperature in this disease, rising and falling, as it does, under very slight disturbing external influences, makes it a matter of surprise not that the temperature is not always typical, but rather that it ever closely conforms to the type.

"Second, the inversion of the diurnal range, of which Dr. Hollopeter has given two instances, is not common. I have seen cases of it, to which I will briefly refer. I am not aware that any adequate theory of this curious clinical phenomenon has yet been advanced.

"Third, house-epidemics. The second part of the paper, which considers the details of this curiously circumscribed and localized outbreak, is very interesting and important. It is by the study of such house-epidemics that a true insight into the etiology of the disease is to be gained. I do not agree with the author of the paper in ascribing the disease in the infected house to the percolation of waste-water through the cellar-wall from the adjoining premises. In order to establish this theory it would be necessary to show, first, that such percolation, if capable of producing this disease, had not been going on for an indefinite time prior to the attack, and, secondly, that typhoid fever had existed in the neighbor's house, or at least that the dejecta of typhoid patients had been thrown into the pit from which the drainage into the cellar ran. These facts have not been shown. On the other hand, it appeared clear enough that the first case, the young woman who had come home sick from another house, had introduced the disease. An investigation of the house and neighborhood from which she came would doubtless shed light upon the subject. House-epidemics are numerous in Philadelphia. Certain houses, however, seem to be the abiding place of the infecting principle, seeing that cases occur in them from time to time during a series of years."

Dr. E. T. Bruen: "I have been very much interested in Dr. Hollopeter's paper, and also in the remarks of Dr. Wilson. I agree particularly with the portions relating to the contagion. Two cases recently seen by me illustrate this. A washerwoman took home the clothes of a typhoid-fever patient. Within a week her two children drooped and became feverish. They were taken to the Children's Hospital, and one died there with no autopsy. The other was taken home, where it died,

and I made a post-mortem examination. The typical conditions were present. No other cause for their sickness was known.

"I am also interested in what Dr. Wilson has said about alterations of temperature from slight causes. We notice the same thing in diseases of the liver, in which cases the thermal wave fluctuates with the ingestion of food. If we admit these causes, I think we can readily admit psychic influences in the same rôle.

"In diagnosing typhoid fever, it is to be distinguished from malarial fever and catarrhal fever. Malarial fever is *sui generis*, and can be controlled in this latitude by quinine. I believe that malaria will modify the initial stages of typhoid fever. Thus one of the groups of cases called typho-malarial takes its name. Neglected malarial fevers form another group of so-called typho-malarial fever, which is not a disease *sui generis*.

"I have seen many cases of mild typhoid fever,—the so-called typhoid ambulatorius. Throughout these cases have shown a temperature of 100°, not over 100°, but I would not make the diagnosis of typhoid fever if the temperature after the fifth day fell below 100° in the evening.

"Catarrhal fever is characterized by peculiar weakness, with various catarrhal conditions of lungs and bowels. The temperature runs from 99° to 100°, and is at times almost normal. Such cases should be classed as catarrhal fever, and are very similar to the specific forms of influenza. They may be recognized by the fact that the temperature is very irregular, and it is impossible to control this feature by quinine. I think these are the cases which have often been confused with mild typhoid."

Dr. Baldwin: "Dr. Levick some time ago published a paper in which he spoke of a family predisposition to typhoid fever as well marked in some families as that of phthisis. I should like to ask Dr. Wilson if he has noticed any cases of like character."

Dr. B. Trautmann: "Niemeyer speaks of inverse temperature in typhoid fever, and says that these cases end generally fatally. This seems to disagree with the statements made here to-night. I have at this time a case showing this curve, in which a hemorrhage of the bowels followed this morning."

Dr. Kevin: "I have also a case now under treatment showing inverse temperature."

Dr. Arthur V. Meigs: "With regard to the etiology, I do not think Dr. Wilson's position is well taken. In the face of the authority of Murchison that typhoid fever may arise *de novo*, it seems to me hardly fair to assume the germ-theory as proved, although the medical mind seems at present to be inclined in that direction. Typho-malarial fever has always seemed to me a bad name, for it misleads; it means in its proper sense a hybrid,—both malarial disease and typhoid

fever co-existing in the body at the same time."

Dr. W. A. Edwards: "In the case of a young man under my care whose temperature presented the peculiarities spoken of, a diagnosis of double quotidian was made. On the eighth day signs of typhoid fever set in, and lasted four weeks, then again there was a typical double quotidian. If the case had not been so diagnosed, it would have been put down as a case of typhoid fever with inverted temperature. This, with other cases, I published in the *Medical and Surgical Reporter*, November 17 and 24, 1883, and mention it to-night to call attention to the fact that all cases of inverted temperature in typhoid fever are not due to the typhoid entirely, but frequently to a complication, this complication occasionally being almost unrecognizable except for its effect on the temperature."

Dr. Wilson: "A clear comprehension of the mode in which typhoid fever is propagated from previous cases by means of a specific infecting principle is of the utmost importance. Murchison, it is true, held that it might arise *de novo*. Nothing in the literature of the subject is more brilliant than that great author's advocacy of this view, and the student of the subject knows that it was maintained in Murchison's day with a vigor scarcely inferior to that with which it has been defended here, with more success. To Dr. Wilson Budd, of Bristol, is due the credit of showing that Murchison's view was untenable. To-day it is no longer entertained."

Dr. Meigs: "I did not pretend to deny that a single case of typhoid fever may be the focus from which any number of others may arise. I merely contended that, in the face of Murchison's opinion that the disease may arise *de novo*, we should be careful how we assume as proved the germ-theory."

Dr. J. Solis Cohen: "I do not rise to speak directly to the subject of the paper, as I do not see typhoid fever except in hospital practice. Many years' observation has convinced me that sudden rises of temperature in typhoid fever, as in other diseases, are often due to obstruction of the bowels, even though diarrhoea may exist. A small dose of castor oil will usually quickly relieve the bowels and reduce the temperature. The ordinary milk-diet is then to be modified by the addition of lime-water, or by substituting boiled milk for cold. My residents have for years had general instructions to pursue this plan without awaiting my daily visits.

"The family epidemic recorded reminds me of a similar one in a German family, the members of which were distributed to several hospitals, three of them coming under my own care. A marked local epidemic occurred among the sailors on board the Russian war-vessels which several years ago were lying off the wharf at Kensington, some sixty of

whom came under my care with the most typical demonstration of the true typhoid curves that I have ever seen. The temperature-charts looked as though they might have been copied from a text-book. The cause in these instances was the drinking of water from the Delaware River, which the sailors, following an old custom, dipped from the side of the vessel to within a few feet of the open outlets of sewers.

"I was very much interested in the story of the poor washerwoman's family. Our duty in the presence of contagious disease is very clear, and is too much neglected by physicians. We should not allow our patients to send clothes out to be washed until they have been thoroughly disinfected. Indeed, rather than subject a family to contagion, let the clothes be burned. Many a poor washerwoman in this city brings typhoid fever, diphtheria, and other diseases to her household through the carelessness or indifference of her customers."

Dr. Hollopeter, in closing the discussion, said, "I had felt a certain uneasiness in presenting my cases of *inverse temperature*, as I could obtain but little authority for the same; yet I have recorded the simple facts, and am glad to learn that other members have also noticed similar cases. In a future paper I hope to give more of the literature of the subject. In answer to Dr. Wilson as to the causation of the house-epidemic, I would state that Mary, the first to take sick, was at the time not living at home, yet she frequently came home for a few days at a time. The family she was working in were all healthy."

PHILADELPHIA NEUROLOGICAL SOCIETY.

THE regular stated meeting was held December 22, 1884, the Vice-President, Dr. CHAS. K. MILLS, in the chair.

Dr. MILLS read a paper on the treatment of epilepsy, of which the following is an abstract:

NOTES ON THE TREATMENT OF EPILEPSY.

His remarks applied chiefly to the medicinal treatment of epilepsy proper,—grand mal and petit mal. He did not intend to refer to the treatment of convulsive seizures due to tumors, meningitis, spinal disease, etc.

As to the curability of epilepsy, some apparently authentic cases of cure were on record. After looking over the subject, he could only recall seven cases of apparent or real cure in his own practice, private and hospital. In all of these the attacks remained away for from more than one to more than three years. Gowers mentions a number of cures,—at least, cases in which seizures had not recurred for as much as four, five, six, or seven years. Several of these cases were children. Three

of the seven cases alluded to by Dr. Mills were children. All observations as to the cure of epilepsy were necessarily imperfect. After a number of years the attacks recur again. Although, however, the absolute curability of epilepsy might be doubtful, certainly great benefit from treatment is to be derived in most cases. It is a wrong to such patients not to treat them.

He had used the following remedies, singly, three or four times daily, in a series of cases, during the last ten years: Bromide of potassium, grs. x to 3j; bromide of sodium, grs. x to 3j; bromide of ammonium, grs. x to 3ss; monobromide of camphor, grs. iij to vj; hydrobromic acid, f3ss to f3ss; iodide of potassium, grs. v to 3ss; biborate of sodium, grs. xv to xx; chloral hydrate, grs. x to xv; oxide of zinc and valerianate of zinc, grs. iij to x; nitrate of silver, gr. $\frac{1}{4}$ to gr. $\frac{1}{2}$; tincture of belladonna, ℥x to xv, or extract of belladonna, gr. $\frac{1}{4}$ to gr. $\frac{1}{2}$; extract of cannabis indica, gr. $\frac{1}{4}$ to gr. $\frac{1}{2}$; fluid extract of cocculus indicus, ℥j to iij, or tincture of cocculus indicus, ℥v to x; nitrate of potassium, grs. v to viij. Other remedies, which he had only used in combination with some of the above drugs, were as follows: Conium juice, f3j, or fluid extract, ℥iij to x; tincture of digitalis, ℥v to x; sulphate of strychnia, gr. $\frac{1}{8}$ to $\frac{1}{4}$; Fowler's solution of arsenic, ℥iij to v; fluid extract of ergot, f3j to f3ij, or extract of ergot (Squibb's), grs. iij to vj; iron, cod-liver oil, and quinine.

The bromides were certainly the best remedies, and the bromide of potassium, in his opinion, stood at the head of the list. The bromide of sodium came next best to the bromide of potassium. He thought the bromides affected the nerve-centres directly. The mixed bromides were better, usually, than any one of them used singly.

The bromides could be advantageously combined with other drugs. The combination which he had found, on the whole, the best for long-continued use was bromide of potassium, grs. xv; bromide of sodium, grs. xv; solution of the arsenite of potassium (Fowler's solution), ℥iij; conium juice, f3ss, or the fluid extract of conium, ℥iij to v. This was generally made up with syrup of orange and some bitter infusion.

Another good combination, mentioned also by Gowers, was the bromide of potassium and tincture of digitalis. It was only especially valuable in cases complicated with weak heart or mitral disease. Monobromide of camphor had no advantage over the bromides. Hydrobromic acid was efficient in very large doses, but so much water had to be given with it that the amount to be swallowed was appalling to the patient. It sometimes irritated the stomach. Borax was not of established value, but helped a few cases temporarily. He found the iodide of potassium, unless especially indicated, had little

value when used alone in the treatment of epilepsy. Chloral hydrate is not of much value when given alone, but with the bromides, in cases uncomplicated with cardiac disease, it sometimes makes a useful combination. Trouseau's belladonna treatment had been used without noteworthy success. Cannabis indica was not to be depended upon. He had tried cocculus indicus in six cases at the Philadelphia Hospital. None improved, and four were made worse by its use, one of the four becoming insane while taking the drug. At the request of Drs. Reichert and Hinsdale, he had used the nitrite of potassium in seven cases. Of these, only one seemed to be benefited, and some were made worse by its use. When the bromides had to be stopped, the zinc salts or the nitrate of silver were the best substitutes, but they could only be relied on for a short time.

The way to treat epilepsy, in his opinion, was to simply have a plan of treatment, and carry it out over a series of months or years. He would, for instance, first put a patient upon a single bromide, say fifteen grains three times a day, to be increased until a decrease in the number and severity of the paroxysms was produced. He kept him upon this perhaps for a month. He then used the mixed bromides, or some combination of bromides with other drugs, preferably the bromides, arsenic and conium prescription. He watched the condition of the patient, and, if necessary, also put him on cod-liver oil, quinine, or iron.

With reference to nitrates, bromates, etc., he said that there was probably some chemical or chemico-physiological reason for inefficiency. The *ites* and *ates* would probably never give as good result as the *ides*. Dr. Mills believed with Dr. Pepper (*Med. and Surg. Reporter*, January 12, 1884) that close attention should be paid to every point in the daily life of an epileptic,—to diet, rest, and hygiene; but he did not, with his present experience, believe that a genuine case of thoroughly-developed epilepsy could be cured, or even greatly benefited, without drugs. Harm, however, might be done by over-use of drugs.

Among the surgical and external means of treatment he had successfully used were excision of cicatrix, removal of neuroma, actual cautery, and blistering to the neck or head. He said he did not use counter-irritation to the scalp, but was a strong believer in the actual cautery, used after Brown-Séquard's method, to the nape of the neck.

DISCUSSION.

The discussion was opened by Dr. E. T. REICHERT. He said that the efficacy of the bromides was certainly not due to the bromine, for bromide of potassium is the best of the bromides, and it contains less bromine than any of the others. The amount of

bromine in the chief bromides, as given by Gowers, is as follows:

Bromide of ammonium . . .	81 per cent.
Bromide of potassium . . .	67 "
Bromide of sodium . . .	77 "
Bromide of lithium . . .	92 "

All the bromides act alike, but with varying degrees of efficiency. The bromates, nitrites, etc., are not as useful as the bromides. A certain line of similar chemical salts seem to give uniform physiological and therapeutical results.

Dr. GUY HINSDALE said that, in regard to the cases in which Dr. Mills used the nitrite of potassium, some four years ago, one was benefited, some remained the same, while others were apparently made worse by its use.

Dr. WHARTON SINKLER wished to know if any of the members of the Society had used hyoscyamus, and with what results. He said that Althaus, of London, reported cases in which tincture of hyoscyamus had been used successfully in doses of from one, two, or three drachms. He said that he had reported one case of epilepsy which had been greatly benefited by cannabis indica, after the bromides and other remedies had failed.

Dr. H. M. WETHERILL said that, in his experience, the bromide of potassium was the best remedy. He said he had never tried the tincture of hyoscyamus. He had never seen a case of epilepsy entirely recover.

Dr. F. DERCUM said that his experience was summed up in the statement that he had found the bromides the best.

Dr. MORRIS LEWIS spoke of the cure of a child 3 years old, at the Episcopal Hospital, in which he could obtain no results with the bromide of potassium. The seizures continued, and were worse, if anything. He then resorted to bromide of ammonium, and with immediate good results.

Dr. J. H. LLOYD said he thought that in cases of epilepsy attention to the digestive functions and to sleep was highly important. He called attention to the observation of Dr. Radcliffe, that epileptics have a tendency to overeat and to sleep too much. In his own experience he had found that when constipation was present the seizures were likely to be more frequent and severe. Dr. Radcliffe and other English physiologists seemed to prefer comparatively small doses of the bromides long continued. Dr. Lloyd believed the combination of three bromides with the iodide of potassium to be one of the best in the treatment of epilepsy.

Dr. A. J. PARKER said he believed we could not say that the effect of a drug like bromide of potassium, bromide of sodium, etc., was due either to the bromine, or potassium, or the sodium, etc. Each of these salts is an individual drug, each molecule of which has its own form of motion. Bromine is a sub-

stance with its own peculiar molecular condition of motion. Potassium, similarly, has its own motion. Bromide of potassium is the resultant of the two motions. Certain members of the same chemical families, as the bromides, iodides, and chlorides, would seem to go together in therapeutical efficacy, due to similarity of their forms of molecular motion. At any time we may expect, however, to find wide differences, since our chemical equations simply represent an idea of structure, and tell us nothing concerning the form of molecular movement; and therapeutical effects must certainly depend more on the energy and form of motion than upon any other qualities.

Dr. E. T. REICHERT made a few remarks on the use of

HYDROBROMIC ACID AND THE BROMIDES IN SEA-SICKNESS.

He said, while crossing the ocean, he had the opportunity of studying the effect of hydrobromic acid and some of the bromides in sea-sickness. Hydrobromic acid was effectual, but had to be used in such large doses that, owing to the dilution with water necessary, the bulk to be taken was excessive. The bromides were therefore, he thought, to be preferred. Bromide of sodium, in large doses, was effectual in one bad case. He believed that all the bromides were good.

Dr. J. M. TAYLOR spoke of the use of

NITRITE OF POTASSIUM IN ANGINA PECTORIS.

In an aggravated case of angina pectoris he had obtained very good results with nitrite of potassium. The patient had been suffering for over a year with paroxysms of increasing intensity, until death seemed imminent. After giving the nitrite of potassium for four weeks the patient recovered. Nitrite of amyl was used sparingly, to control the paroxysms.

LEWIS BRINTON, M.D.,
Recorder.

PHILADELPHIA CLINICAL SOCIETY.

At a stated meeting held November 28, 1884, Dr. HENRY BEATES in the chair, Dr. ROBERTS read a paper entitled

NOTES ON SOME INSTRUCTIVE CASES OF HERNIA.

He reported cases illustrating reduction of an irreducible femoral hernia of seven years' standing, irreducible incomplete hernia, recovery from strangulated hernia without operation, herniotomy for strangulated femoral hernia in a paralytic, and others. He desired to emphasize particularly the following points:

That inguinal and femoral hernia will be found at the same time on the same side much oftener than is supposed.

That it is safer to operate in cases of sus-

pected strangulation than to postpone operation beyond twelve hours.

That herniotomy is attended with little hemorrhage, and, if done antiseptically, is accompanied by rapid union and little risk to life.

DISCUSSION ON DR. ROBERTS'S PAPER.

Prof. JAS. B. WALKER: In connection with Dr. Roberts's paper, a brief narration of the salient points of the following cases may be of interest:

Case I.—A lady, aged 79, as the result of a fall noticed a swelling in the left groin below Poupart's ligament, in the vicinity of the femoral ring. The tumor soon occasioned marked disturbance. It was of doughy consistency, and cough failed to impart succussion. Percussion did not elicit a tympanitic note, and taxis failed to reduce it. As there was a doubt of its real nature, anæsthesia was induced with ether, and still taxis failed. The existence of symptoms of strangulation—viz., constipation, nausea, and vomiting—determined operative procedure, when an entero-epiplocele was disclosed. The omental element was the greater, and enclosed a small knuckle of gangrenous intestine; this constituent of the tumor was so small that the ordinary symptoms of succussion and tympany were rendered non-detectable. Hot water failed to develop manifestations of vitality, so the section of intestine was excised and the healthy ends stitched together. Suppression of urine followed the operation, and death resulted on the third day.

Case II. was an instance of inguinal hernia in a female advanced in years. Efforts at reduction by a number of physicians had failed. Notwithstanding the length of time intervening, by properly applying taxis, he succeeded in reducing the tumor. The probable cause of failure on the part of other attendants was improper attempts at taxis. The mass pressed out over the ring in a manner to defeat their object. This is, in a majority of instances, the undoubted cause of failure of efforts at reduction.

Case III.—A male with complete right inguinal hernia, strangulated. Taxis failing, an operation was resorted to, but the case terminated fatally. The peculiarity consisted in a deposit of fibrin or lymph nearly two inches in thickness surrounding the external ring.

Case IV.—A gentleman, whose vocation required at times heavy lifting, complained of a peculiar burning or stinging pain limited to the right inguinal region. There was no perceptible tumor. The symptoms were invariably aggravated by physical effort. I diagnosed incipient inguinal hernia, and advised a truss. The patient, being dissatisfied, consulted an irregular physician, who ridiculed the diagnosis given, and administered homœopathic powders for nearly four

weeks, with, of course, no result. By this time the hernia was protruded and unmistakable. The patient now wears a proper truss, and is practically well.

In reference to umbilical hernia, I wish to call attention to the fact that the umbilical opening is comparable to the cranial fontanelles in that, like the latter, it gradually closes with advancing age. Basing my plan of treatment on this physiological fact, I support the abdominal walls with a broad adhesive strip so applied as to approximate the umbilical region. To the umbilicus I adjust a compress smaller than the opening. This prevents protrusion without interfering with the closing process.

Dr. J. G. HEILMAN narrated the history of an aged colored man who suffered from an inguinal hernia, which became strangulated. Anæsthesia with chloroform did not render reduction practicable. Drs. E. R. Stone and L. B. Hall were called in, and, at the suggestion of the former, about a fluidounce of a straw-colored fluid was evacuated from the sac by means of an ordinary hypodermatic needle. After this procedure the hernia was easily reduced, and a favorable result followed.

Dr. L. BREWER HALL directed attention to the high prices asked by instrument-makers for trusses, and inquired if there was any way by which the poor could get them at more reasonable rates.

Dr. WALKER, in reply to Dr. Hall, explained how a ten-dollar truss could be obtained from a Chestnut Street dealer, under such circumstances, for seven dollars.

Dr. ROBERTS, in closing the discussion, said, "In my opinion, the mortality in cases of hernia is due to two causes,—first, too great an effort at taxis, whereby inflammatory changes are occasioned and surgical treatment complicated and endangered; second, delay. If gentle taxis fails, herniotomy should promptly be performed. The conditions of delay and experimental manipulation by students in hospitals render hospital statistics of little value. In one case of double hernia (*i.e.*, an inguinal and femoral hernia on the same side) I succeeded in maintaining them in place by an inguinal truss so depressed as to occlude both the external abdominal ring and the femoral ring. In umbilical hernia I apply a compress moulded of wax instead of the coin. It is more readily adapted to the size of the orifice and kept in close apposition."

Dr. STONE'S paper was then read (see page 309).

DISCUSSION ON THE MANAGEMENT OF LABOR.

Prof. J. B. WALKER agreed so fully with the points advanced by Dr. Stone that discussion seemed almost superfluous. He believes that the method described of hooking the placenta from the uterus with the finger is

one in which there is considerable danger of introducing disease-germs, because of carelessness and want of cleanliness on the part of accoucheurs. Therefore, for a general rule, it should be taught that expression should be persevered with until the placenta is forced into the vagina. Hot injections of pure water he believes to be especially useful, and the dangers following this practice, so emphatically declared by hospital experience, are a myth, and should not deter private practitioners from their employment. In hospitals, where one syringe serves for many cases, there is danger, but in private practice, where new instruments can be had and their use limited to the individual, danger does not obtain; on the contrary, the risk of infection by a nurse whose clothing may have been subjected in previous cases to septic poison is by this plan of treatment greatly diminished. Again, these injections of hot water overcome inertia. They exert a powerful oxytocic influence and promote favorable physiological processes. If these injections during the lying-in period fail to cause firm uterine contraction, quinia should be administered.* The use of ergot in the third stage of labor is a moot question possessing so much of *pro* and *con* that I do not feel in a position to state invariable rules regarding its employment.

Dr. E. MONTGOMERY thoroughly indorsed Dr. Stone's paper. He calls especial attention to the great importance of resorting to diagnosis of the presentation and position by external palpation performed during the last month of gestation. He has recognized faulty conditions and been able to correct them, thus securing the advantages of a normal labor to both mother and child. He makes it a rule to visit his patients during their last month, diagnosticate the position, and at the same time ascertain the general condition. The unusually small number of lacerations of the perineum is a conspicuous feature in the doctor's experience, and argues strongly the skill attained and the commendable care with which he conducts his cases. The position described in which the obstetrician should place himself in order to command better control of the patient during the period at which the head and shoulders are being extruded is one of great advantage, and one which enables the attendant to avert this lesion in many instances. Cervical lacerations are of very frequent occurrence. Many spontaneously heal to such a degree as to be practically cured. Traction on cord combined with fundal compression is my method of delivering the placenta. A twisting movement is always a valued resort, it converts the

membranes into a rope like mass, which reduces the possibility of tearing and leaving portions within the uterus to a minimum, and maintains firm uterine contraction, which is always followed by excellent results. Ergot should not be administered as a routine method, as many conditions may be present the treatment of which the effects of this drug would compromise: adherent placenta is an instance. Regarding anæsthesia, the prompt and transitory effects of ethyl bromide, and its freedom from danger when used as obstetric conditions demand, render it an especially valuable drug. It does not interfere with uterine contractions, is not followed by inertia or unpleasant effects. Several instances of sepsis have occurred in my private practice that cannot be traced to hospitalism or to attendance upon diseases that supply the septic poison, as scarlatina, erysipelas, etc. As a consequence, he always employs corrosive mercuric chloride, cleansing his hands with it before making the necessary examinations and manipulations. After accouchement, he directs the vulva to be covered with a pad saturated with a solution of the chloride. This is changed about thrice in twenty-four hours. Since resorting to this he had not had one case of septic disease occur in private practice, and in that hot-bed of septic poison, the Philadelphia Hospital, where every year he formerly had many cases of puerperal fever, but two cases occurred. The practice of injecting the vagina in a hospital is apt to serve as a means of introducing sepsis.

Dr. ROBERTS called attention to pellets of corrosive sublimate, prepared by a pharmacist, that serve as a convenient form for carrying and having for use this remedy. He has not had experience with ethyl bromide in light anæsthesia, and cannot say whether or not, used in this way, it is a safe drug. In the profound anæsthesia required in surgery his experience compels him to regard the drug as not free from danger.

Dr. STONE, in closing the remarks, detailed the conditions of laceration and the results of non-surgical but careful treatment, which, as the results were practically those of cure, rendered the percentage-report apparently unusually favorable.

CHICAGO GYNÆCOLOGICAL SOCIETY.

REGULAR MEETING, DECEMBER 19, 1884.

The President, H. P. MERRIMAN, M.D., in the chair.

DR. W. H. BYFORD read a paper entitled

A CASE OF MURAL PREGNANCY.

The history of the case was obscure. The patient, 28 years old, married seven years, had one child, six years old. She supposed

* I have never in private practice seen a single case of septic disease induced by injections. Hot water alone is all that is necessary. In one instance, where the phenomena of sepsis were developing and decidedly established, the symptoms were promptly relieved by hot water alone. J. B. W.

that she became pregnant for the second time in February, 1883. In April, after being fatigued, she had hemorrhages, which continued until May 9, about four weeks. October 14 a discharge of yellow fluid, about one gallon in quantity, occurred. A putrescent, sero-sanguineous discharge followed, continuing three months. January, 1884, a large brownish mass with very fetid odor was expelled. After this event menstruation occurred until July. In May she was quite large and had bearing-down pains. She entered the hospital October 6, 1884. She was tapped October 18, and about four quarts of thick, tenacious fluid, resembling the fluid of an ovarian cyst, were removed. This fluid coagulated on the addition of nitric acid and on boiling. Assisted by Dr. R. Filley, a microscopic examination was made, with negative results. The "Drysdale" cell was not found. Laparotomy was performed, and a fœtus with placenta was removed without hemorrhage or difficulty. In order to secure perfect drainage it was considered best to remove the uterus. The operation was performed on October 30. The patient did not react, but died within twenty-four hours. Prior to the operation the patient was extremely reduced by her protracted sufferings. Dr. Byford in a similar case at the present time would elect the vaginal operation. The specimens removed from this woman were exhibited as supporting the diagnosis of mural pregnancy.

This was the second case of mural pregnancy that had come under the reader's observation within a period of five years. The first case was reported to this Society some time ago. The patient was in labor and moribund when Dr. Byford saw her. She had been in labor until exhausted. There was no difficulty in making a diagnosis. The head was low down in the pelvis, almost on the perineum. The os uteri was wellnigh inaccessible behind and above the symphysis. The body of the uterus, somewhat enlarged, could be felt in the lower and anterior part of the abdomen, attached to the tumor containing the fœtus. The fœtus could be felt through the abdominal walls, surrounded by a thick involucre apparently as thick as the uterine walls. Fœtal extremities could be distinguished. When dissected, the sac in which the fœtus was contained was found to consist of a thick layer of muscular fibres. These fibres were directly continuous with those of the uterus. The tubes and ovaries lay upon the sides of the lower portion of the sac. The fecundated ovum had made its way down the tube, became lodged in a diverticulum in the uterine wall, and was gradually extruded into the cavity of the abdomen. The fœtus was thus developed within the uterus, though not within the uterine cavity. The resemblance to normal pregnancy was great in the presentation and position of

the fœtus, deep down in the pelvic cavity, behind the vagina. The head in this case was fixed by the concentric contraction of the uterine fibres by which it was surrounded, and could be easily outlined as it lay thus covered by the posterior vaginal wall.

The specimen presented is much less perfect than the one described, because of the numerous effects wrought upon it during the great length of time it remained in the maternal body and the mutilation consequent upon enucleation.

The treatment of these cases ought to be considered apart from that of extra-uterine pregnancy at term. It is always a matter of special consideration, in connection with each case as it presents itself, whether or not the removal of the fœtus at term in extra-uterine gestation should be attempted. The dangers of laparotomy are greatly increased by the inability to remove the placenta. The surface to which it is attached has no contractile power, so that the divided vessels are left patulous. If hemorrhage does not immediately prove fatal, the blood is a source of sepsis that must almost certainly destroy the patient. Laparotomy would more likely prove successful if performed some days after the death of the child. In these cases of ectopic or interstitial uterine pregnancy the fœtus may be easily removed through the vagina. An incision made through the posterior vaginal wall would completely uncover the presenting part and enable one to apply the forceps or attack it with the perforator and crotchet as in ordinary labor. After the removal of the fœtus the placenta should be allowed to separate spontaneously.

Since writing this report, Dr. Byford had seen a case reported in the *Annales de Gynécologie*, July, 1884, occurring in the practice of Mr. Matheson, of England, illustrative of the execution of this plan. The case was reported to the London Obstetrical Society under the title "Extra-Uterine Pregnancy: the Extraction of a Living Fœtus through the Vagina." The child was slightly asphyxiated, but survived. A sponge saturated with perchloride of iron was introduced into the sac after removal of the placenta. The mother recovered. It would seem that the author did not suspect his case to be one of interstitial pregnancy. During the discussion that followed, only one of those present expressed the opinion that it was of that variety. Mr. Griffith thought it was either interstitial pregnancy or one in which the fœtus was developed in one portion of a double uterus.

A general discussion followed the reading of the paper, in which Drs. Edward Warren Sawyer, E. C. Dudley, J. H. Etheridge, A. Reeves Jackson, John Bartlett, W. W. Jagard, Charles W. Earle, and Dr. Byford participated. Some difference of opinion being expressed, the specimen was referred for ex-

amination to a pathologist, not a member of the Society, for report at the next regular meeting.

Dr. Etheridge presented a specimen of a placenta containing calcareous deposit from a woman whose pregnancy had extended to two hundred and ninety-two days.

W. W. JAGGARD, M.D.,
Secretary.

REVIEWS AND BOOK NOTICES.

CONVERSATIONS BETWEEN DRs. WARREN AND PUTNAM ON THE SUBJECT OF MEDICAL ETHICS: WITH AN ACCOUNT OF THE MEDICAL EMPIRICISMS OF EUROPE AND AMERICA. By FRANK HASTINGS HAMILTON, M.D. Birmingham & Co., New York, 1884.

The vexed question of the utility of the National Code of Ethics has been so diligently urged in New York State as, in effect, to divide the profession of the Empire State into two partisan camps, with a state of feeling existing between the two which is truly deplorable and highly discreditable. It has led to the establishment of rival county medical societies and a rival State medical society, by which the breach between the two parties will be perpetuated. One of the peculiarities of the new-code, or no-code, advocates is their refusal to discuss medical ethics from any higher point of view than a purely business standard, and consequently they are impervious to most of the arguments of their opponents. Were this not the case, we think that this admirable publication of Dr. Hamilton and the writings of Drs. Flint and Squibb would be sufficient to re-establish the authority of the Code. The series of interesting supposed conversations of Drs. Putnam and Warren completely cover the ground upon which the Code is founded, and fully maintain its teachings and answer the objections which have been raised by its critics; but, in conclusion, the author suggests that certain amendments might be made which, without altering its spirit, would increase its efficiency and acceptability.

YEAR-BOOKS OF MEDICAL PROGRESS.—A YEAR-BOOK OF THERAPEUTICS FOR 1883, Edited by ROYAL W. AMIDON; and A YEAR-BOOK OF SURGERY FOR 1883, Edited by CHARLES H. KNIGHT, M.D. G. P. Putnam's Sons, New York, 1884. Cloth, 8vo.

These handsomely-printed volumes furnish a well-selected and classified digest of medical and surgical contributions to the literature of the science in the year 1883. The matter is conveniently arranged for reference, and, being well edited, the works are both convenient and useful. To those who are engaged in the almost vain effort to keep up with cur-

rent medical literature these year-books furnish comfort and satisfaction, as they represent an annual taking of stock, and a sifting of the wheat from the chaff. The surgical volume has no index, but its table of contents and careful arrangement under sub-headings may render an index unnecessary. An index of authors would facilitate reference, and might sometimes prove very useful. Bibliography has not been attempted to any great extent, as that is furnished by the "Index Medicus," to which these volumes are useful supplements.

THE ELEMENTS OF PATHOLOGY. By EDWARD RINDFLEISCH, M.D. Translated from the First German Edition by W. H. MERCUR, M.D.; Revised by JAMES TYSON, M.D. Philadelphia, P. Blakiston, Son & Co., 1884. 12mo, pp. 263.

The subject-matter of this work is an outline sketch or groundwork of pathology, without having any pretensions as a text-book. Having this in mind, the various sections of the (a) general part, on (1) the Local Outbreak of Disease, (2) the Anatomical Extension of Disease, (3) the Physiological Extension of Disease, followed by (b) Animal Disturbances, and the special part, including Traumatic, Parasitic, and Infectious Diseases, Defective Development and Growth, Diseases due to Overwork, and Diseases of Involution, may be read with interest and profit. The views of Rindfleisch will command attention, if not assent, in every particular, and it is not to be expected that his views will command universal acceptance. It is therefore a book more for the practitioner than the beginner, and presupposes some familiarity with the text-books upon the subject. The work of translation has been well done, although we notice some obscurities of expression which might be improved.

THE PHILADELPHIA MEDICAL REGISTER AND DIRECTORY FOR 1884-5. Edited by WILLIAM B. ATKINSON, A.M., M.D. Collins, Printer, 1884.

This well-known physicians' directory, issued under the direct supervision of Dr. Atkinson, preserves its characteristic features which have made it such a useful work of reference in the past, and presents some new ones which enhance its value.

THE *Archives of Medicine* for December contains the editorial announcement of the discontinuance of its publication with the present number, in view of the general preference for periodicals appearing at brief intervals able to furnish scientific material and news with greater promptness. We regret to lose the *Archives* from our exchanges, especially as the last issue contains so much valuable and interesting original material. A general index to the twelve volumes of the *Archives* is also contained in this number.

GLEANINGS FROM EXCHANGES.

EXFOLIATION OF THE COCHLEA WITHOUT CAUSING DEAFNESS.—The *Lancet* Vienna correspondent writes: The loss of the cochlea, according to the ingenious researches of Helmholtz, is considered to be equivalent to the loss of hearing-power. A case, however, which Professor Grüber demonstrated on December 12, at the meeting of the Society of Physicians in Vienna, would seem to go far towards refuting this view. The case occurred in a lad aged 14, who had been ailing for four years, and who was admitted into Professor Grüber's wards last November. It was stated that the patient suffered from an old otorrhœa with polypus of the left ear. As the patient experienced great pain, and as paralysis of the left nervus facialis existed, Professor Grüber had no hesitation in entirely removing the polypus, and in doing so came upon the cochlea in a state of necrosis. Since the operation the patient has felt better, his pains have ceased, and the paralysis of the nervus facialis has nearly disappeared; but, strange to say, the lad's affected ear has regained its power not only of recognizing sounds, but of distinguishing musical notes. Only two similar cases would seem to have been recorded. One was observed by Dr. Cassels at Glasgow, the other by Dr. Christinneck in the clinic of Professor Schwarze at Halle. Professor Grüber did not enlarge on the features of this interesting case, but it certainly affords food for suggestive thought in reference to treatment of diseases of the ear. Heretofore the labyrinth was considered a sort of *noli me tangere*, but now we see that the entire cochlea may be missing and yet the aural powers be retained. On the other hand, it is not yet quite certain whether in the above case paralysis of the auditory nerve may not eventually occur.

TRICUSPID STENOSIS.—In reviewing the general facts of tricuspid stenosis *à propos* of a case of his own, M. Chauffard points out that the only two signs in life we are likely to get of its presence are (1) the existence of extreme venous congestion in the neck without a venous pulse, and (2) presystolic and systolic murmur which culminates over the junction of the xiphoid cartilage with the sternum, and which is by this position and its harshness and superficiality to be distinguished from the murmur of mitral stenosis. With regard to the general conditions of tricuspid stenosis, M. Chauffard is in agreement with nearly all authors,—viz., that it is almost always found in women, in forty-one cases out of forty-six (Fenwick) in adult life, and accompanied by other endocardial lesions, especially by mitral stenosis. In only half the cases does rheumatism seem to have been coexistent; it has been the result of a slow endocarditis, wide-spread and not vegetative,

which has glued the bases of the valves together and made them contract. In only one case does it seem to have been congenital. (*Revue de Médecine*, July, 1884).—*Practitioner*.

TROPHONEUROSIS (?) OF THE TESTICLE.—A case which may possibly be an example of a trophoneurosis is recorded by M. Lemonnier in *La France Médicale* of December 25. The patient was a young man aged 19, who sustained severe injury to the skull after a fall from a height of about six feet. Hemiplegia of the right side, and rapid atrophy of the testicle and of the right upper limb, are the chief phenomena recorded. There was considerable disturbance of consciousness, but ultimately the patient appears to have recovered to a large extent. The hemiplegia was probably dependent upon compression by blood poured out as the result of the fracture of the base of the skull. No other conditions seem to have been present to account for the rapid wasting of the arm or testicle. Nélaton and others have spoken of the possibility of atrophy of the testicle resulting from causes acting at a distance.—*Lancet*.

MISCELLANY.

SOME RECENT DECISIONS ON MEDICO-LEGAL MATTERS.—The records of the courts are full of cases of interest to physicians, and a knowledge of them will often be of great value when called upon to appear as parties in actions or as expert witnesses in cases where other persons are litigating. Of the most recent cases the following are of general interest:

In an Indiana court a physician was charged with rape upon the person of a female patient, and the crime seemed to be proved, but the question arose whether the guilty party could not escape the highest penalty because of a possible consent on her part. There was no evidence produced to warrant the claim of a consent. On this point the court said, "If the jury believed, as they might well have done under the evidence, that the appellant as a physician obtained possession and control of Rebecca's person, under her mother's command, for the purpose of making a further examination of her alleged disease of the womb, and not for the purpose of sexual intercourse, and that she never in fact gave her consent, through fraud or otherwise, to the sexual connection, then it seems to us that the appellant was lawfully convicted of the crime of rape." The verdict of the jury was therefore affirmed.

In a Michigan case, the charter of a city obliged the common council to take measures for the preservation of the public health, and

in carrying out this requirement a nurse was employed to attend a smallpox patient. A State law requires patients in the hospitals, if pecuniarily able, to pay for the services rendered them. The nurse faithfully attended to the case, and presented her bill to the city authorities who had hired her. They declined to pay, saying that she must sue the patient. She refused to take this course, but brought suit against the city, and the courts sustained the action and obliged the city to pay. The opinion of the judges on appeal contains the following comment: "Individuals would not be willing to provide necessities, and serve as nurses and assistants, at the instance of the public, if compelled to collect their pay of the patients or their relatives. The public is first and immediately responsible, and the intent of the statute is to enable the public to obtain reimbursement from those who ought to sustain the expense. The plaintiff hired herself to the city. She trusted the city, and no one else."

In another Michigan case, a tramp was run over by a locomotive in a railroad-yard. A surgeon was summoned to help him, and sent a message to the superintendent and asked if he should do so. The superintendent answered, "Yes." Nothing was said about pay, and, in fact, the superintendent had no authority to bind the railway-company to pay for surgical aid. Under these circumstances the surgeon sued the superintendent, under the theory that he was personally liable. The court held, however, that there was no contract between them of such a character as to make the superintendent liable. The effect of this decision is to exempt both the railway-company and the superintendent from liability, and to oblige the surgeon to look to the tramp for compensation. As "tramps" usually are not men of means, the surgeon probably has been obliged to charge up his services to a general benevolence account.

In a Kansas case, where physicians were called to give expert testimony, the judge charged the jury that such testimony "should be received and weighed with caution," and the question was whether this was a proper direction or not. On this point the court said, "The testimony of experts is to be considered like any other testimony; it is to be tried by the same tests and receive just as much weight and credit as the jury may deem it entitled to when viewed in connection with all the circumstances. We think this is probably as good a general rule as any that could be adopted. . . . While many courts speak disparagingly of some kinds of expert testimony,—that with regard to handwriting, for instance,—yet we think that all courts hold that the testimony of competent medical experts is entitled to great respect and consideration. In the present case, we think the expert testimony of the physicians and surgeons, who were in fact appointed by the

court, and who made a personal and professional examination of the plaintiff's eyes, is entitled to great consideration, and that the court below erred when it instructed the jury that such testimony should be 'received and weighed with caution.'"

In Germany the papers report a recent case where a physician used the old methods of treating wounds, instead of the modern antiseptic dressings, and the patient died under circumstances making it probable that if a different treatment had been adopted his life would have been saved. The physician was sued for malpractice, and convicted, the court holding that "every practitioner should keep himself informed on the accomplished progress of science, and have an exact knowledge of modern systems of treatment. If these had been employed, the patient's life might have been saved: hence the liability for negligence."

LIFE-SAVING FROM DROWNING BY SELF-INFLATION.—Dr. Henry R. Silvester (*Lancet*) makes the ingenious suggestion that the inflation of the cellular tissue of the upper part of the body can be accomplished by making an opening in the mucous membrane, at the junction of the gums and cheek, with a knife or sharp piece of wood, and then filling the mouth with air and blowing it forcibly through the opening into the fascia. Sufficient inflation can be obtained in three minutes to support the body in the water.

THE ANNUAL MEETING OF THE MEDICAL JURISPRUDENCE SOCIETY was held January 13. The following officers were elected for the ensuing year:

President.—George W. Biddle, Esq.

Vice-Presidents.—John J. Reese, M.D., and William N. Ashman, Esq.

Recorder.—J. Solis Cohen, M.D.

Secretary.—Henry Leffmann, M.D.

Treasurer.—Hampton L. Carson, Esq.

BURNING OF AN INSANE ASYLUM.—The daily papers contain an account of another horrible calamity, the burning of the Illinois State Insane Asylum at Kankakee on the 17th instant, in which some twenty unfortunate patients perished. The newspaper report blames the Legislature for the niggardly appropriations, which prevented the proper safeguards and fire-escapes from being provided.

THE COUNTY MEDICAL SOCIETY gave a reception in honor of the retiring President, Dr. William M. Welch, on the 14th instant, on which occasion Dr. Welch read a valuable report upon the early history of vaccination in this country as his annual address.

A GOOD APPOINTMENT.—Dr. Edward N. Brush, of the Utica Asylum, and formerly of Buffalo, has been appointed Assistant Physician in Charge of the Male Insane Department of the Pennsylvania Hospital.

NOTES AND QUERIES.

AN OLD FORM WHICH MIGHT WELL BE REVIVED.—To the kindness of Prof. Osler we owe the following copy of an indenture which was in use in the early part of the century in England, and which seems of sufficient interest to warrant publication. We commend it to the State Medical Society.

"*This Indenture Witnesseth*, That Edward O—, of the town of Falmouth, in the county of Cornwall, by and with the consent of his Father, doth put himself Apprentice to James X., of said town of Falmouth, Surgeon, to learn his Art, and with him, after the Manner of an Apprentice, to serve from the 2nd day of March, Eighteen hundred and Eleven, the full End and term of five years from thence next following, to be fully complete and ended.

"*During* which term the said Apprentice his Master faithfully will serve, his secrets keep, his lawful commands everywhere gladly do. He shall do no damage to his said Master, nor see to be done of others, but to his Power shall let or forthwith give warning to his Master of the same. He shall not waste the Goods of his said Master, nor lend them unlawfully to any; he shall not commit fornication nor contract Matrimony within the said term. He shall not play at Cards or Dice-Tables, or any other unlawful Games whereby his said Master may have any loss with his own goods or others during the said term, without the license of his said Master. He shall neither buy nor sell; he shall not haunt Taverns nor Play-houses, nor absent himself from his said Master's Service day or night unlawfully, but in all things as a faithful apprentice he shall behave himself towards his said Master and all his during the said term.

"And the said James X., for and in consideration of the sum of forty pounds lawful money of Great Britain, one moiety of which to him in hand paid, the other moiety when half the term is complete, the said Apprentice in the Art of Surgery and Physic which he useth by the best means that he can shall teach and instruct or cause to be taught and instructed. Finding unto the said Apprentice sufficient meat, drink, lodging, and all other necessities during the said term.

"And for the true performance of all and every the said Covenants and Agreements, either of the said Parties bindeth himself unto the Other by these Presents.

"In witness whereof, the parties above named to these Indentures interchangeably have put their hands and seals this 22^d day of March, and in the fifty-first year of our Sovereign Lord George III., by the Grace of God of the United Kingdom of Great Britain and Ireland King, Defender of the Faith, and in the Year of our Lord one thousand eight hundred and eleven.

"Signed, sealed, and delivered in the presence of

"I. GRIFFIN."

Three-penny stamp (on the paper).

One pound ten shilling stamp (to the indenture).

OBITUARY.

BENJAMIN FRANCIS SILLIMAN, M.D.—At his residence in New Haven, Prof. Silliman died on the 14th inst., of heart-failure, consequent upon progressive cardiac lesion. One of the most honored pioneers of American science, his labors in connection with the Scientific School of Yale College, which owed its inception to his energy and enthusiasm, and his share in the establishment of the National Academy of Medicine, have rendered him eminent in the annals of science in this country, while his contributions to literature have made his name familiar as household words.

Born in New Haven in 1816, he entered Yale in 1833, and was graduated in 1837. Soon afterwards he fitted up a laboratory for instruction in analytical chemistry and mineralogy, which was afterwards developed into the Yale Scientific School, recognized and adopted as a fourth department of Yale College by the authorities in 1847. In 1845 Dr. Silliman delivered what was probably the first series of lectures on agricultural chemistry in the United States, and in 1849 he was called to the Medical Department of Louisville University, in which he took the chair of Medical Chemistry and Toxicology. It was here that he formed a friendship with Prof. Gross which continued through life. He was one of the guests at the dinner given to Prof. Gross by the profession of this city in honor of his completion of the fifty-first year of the practice of medicine.

In 1854, Prof. Silliman, leaving Louisville, returned to New Haven to accept the chair of Chemistry in the Academic and Medical Departments of Yale College, in which he was the successor of his father. His collection of mineralogical specimens, obtained by him in his early travels across the continent and subsequently enriched by careful selection, was presented by him to Cornell University in 1868.

Dr. Silliman's works, *The First Principles of Physics* and of *Chemistry*, were adopted by schools all over the

country, and are still valued as text-books, while his editorial labors in connection with the *American Journal of Science and Arts*, as well as his many papers on scientific subjects, have left an indelible impression upon American science. He received the honorary degree of M.D. from the University of Charleston, South Carolina, in 1849.

OFFICIAL LIST

OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT U.S. ARMY FROM JANUARY 4, 1885, TO JANUARY 17, 1885.

HEAD, JOHN F., COLONEL AND SURGEON.—Retired from active service by operation of law on January 9, 1885, under provisions of Act of Congress approved June 30, 1882. S. O. 7, A. G. O., January 9, 1885.

PROMOTIONS.

LIEUTENANT-COLONEL JOHN CAMPBELL, SURGEON.—To be Surgeon, with rank of Colonel, December 7, 1884.

MAJOR R. H. ALEXANDER, SURGEON.—To be Surgeon, with rank of Lieutenant-Colonel, December 7, 1884.

CAPTAIN HENRY McELDERRY, ASSISTANT-SURGEON.—To be Surgeon, with rank of Major, December 7, 1884.

APPOINTMENT.

JEFFERSON R. KEAN.—To be Assistant-Surgeon, with rank of First-Lieutenant, December 8, 1884.

McKEE, J. C., MAJOR AND SURGEON.—Ordinary leave of absence still further extended four months, on surgeon's certificate of disability. S. O. 6, A. G. O., January 8, 1885.

BENTLEY, EDWIN, MAJOR AND SURGEON.—Leave of absence extended two months. S. O. 8, A. G. O., January 10, 1885.

TREMAINE, W. S., MAJOR AND SURGEON.—Relieved from duty at Fort Porter, New York.

GIRARD, A. C., CAPTAIN AND ASSISTANT-SURGEON.—Ordered for duty at Fort Porter, New York.

APPEL, D. M., CAPTAIN AND ASSISTANT-SURGEON.—Ordered for duty at Plattsburg Barracks, New York.

GIRARD, J. B., CAPTAIN AND ASSISTANT-SURGEON.—Ordered for duty as post-surgeon, Fort Schuyler, New York Harbor.

HAYARD, VALERY, CAPTAIN AND ASSISTANT-SURGEON.—On being relieved at Fort Schuyler, authorized to avail himself of leave of absence (four months). S. O. 8, Department of the East, January 12, 1885.

ELBREV, F. H., CAPTAIN AND ASSISTANT-SURGEON.—Sick-leave still further extended six months, on surgeon's certificate of disability. S. O. 9, A. G. O., January 12, 1885.

SHANNON, WILLIAM C., CAPTAIN AND ASSISTANT-SURGEON.—Relieved from duty at Fort Bridger, Wyoming, and assigned as attending surgeon at Headquarters Department of the Platte. S. O. 2, Department of the Platte, January 5, 1885.

ROBINSON, S. Q., CAPTAIN AND ASSISTANT-SURGEON.—Assigned to temporary duty at Portland, Oregon, from December 17, 1884. S. O. 206, Department of the Columbia, December 22, 1884.

APPEL, A. H., CAPTAIN AND ASSISTANT-SURGEON.—Granted leave of absence for one month, to take effect on or about January 7, 1885 (Madison Barracks, New York). S. O. 268, Department of the East, December 31, 1884.

WALES, P. G., FIRST-LIEUTENANT AND ASSISTANT-SURGEON.—Relieved from duty at Vancouver Barracks, Washington Territory, and ordered to return to his proper station, Fort Coeur d'Alene, Idaho. S. O. 204, Department of the Columbia, December 19, 1884.

LIST OF CHANGES OF STATIONS OF NAVAL MEDICAL OFFICERS FROM JANUARY 3, 1885, TO JANUARY 10, 1885.

Surgeon J. W. ROSS, detached from the "Monongahela," for treatment at Naval Hospital, New York.

P. A. Surgeon HOWARD WELLS, detached from Naval Hospital, Philadelphia, Pa., to the "Monongahela."